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EXPERIMENTS WITH NEW APPARATUS ON JOURNAL FRICTION AT LOW VELOCITIES.

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READ JUNE 4TH. 1884.

WITH DISCUSSION.

The following experiments were undertaken by the writer in the winter of 1878, primarily to test the correctness, especially in respect to initial friction at low velocities, of a series of other tests of rolling stock resistances, made in a totally different manner, on the Lake Shore and Michigan Southern Railway, under the direction of Charles Paine, Member and ex-President of the Society, who kindly furnished the writer all necessary facilities. A report of these tests was published in the Transactions of the Society for February, 1879, but it may be repeated here that the mode of test was by what may be called the gravity or "drop test,"

starting cars from a state of rest down a known grade and deducing the resistances from the velocity acquired. By the aid of appropriate electrical apparatus, which was perfected after some experimenting, time-records were obtained which were easily read to one-hundredths of a second, with a probable error of one or two hundredths of a second ; and the probable error in computation was thus reduced to a very small margin—certainly less than 0.1 pound per ton of resistance. As the number of tests made was also great, so as to constitute an effective check upon the only possible source of error, carelessness or oversights in computation, the probability of any essential error was, in reality, very small. Nevertheless, the results obtained differed so widely from the expectations of the writer, and from the then supposed laws of friction, that some different and direct mode of test seemed desirable as confirmatory evidence. A further motive for making them was the dissatisfaction of Mr. A. Higley, inventor of the Higley roller-journal bearing, in extensive use on horse cars, and then under test for railroad cars on the Lake Shore and Michigan Southern Railway, who was disappointed at the small advantage shown for his patent in the drop tests, and who requested that such secondary and independent tests should be made.

It may be further premised, that in the drop tests referred to there were 11 electric stations, and that the passage of each wheel in the car or train undergoing test over each station left its record on the record tape alongside of the record of a pendulum beating seconds, the pendulum being used because it had been found impossible to give an absolutely uniform feed to the tape. In this manner, data for computing resistances in each test for 15 to 20 different and gradually increasing velocities were obtained, and at the lower velocities especially the data were superabundant.

Constant occupation with other duties has prevented the writer heretofore from making any formal presentation of the result of the direct tests to be here described.

The Apparatus.

The apparatus used is shown with sufficient clearness in Fig. 1. It is extremely cheap and simple, but fulfills its purpose as perfectly as could be desired, and is believed to be entirely novel. The axle *A* to be tested is placed in an ordinary lathe, having as great a variety of speeds as possible. The testing apparatus, as actually constructed,

consisted of an oak beam, *C*, about 4″ x 4″ in size, and about 5 feet long, carrying the compound lever, *L* L^1, each of which multiplies the load applied about 11 times, or, in the aggregate, 125 times. The yoke *E* encircles the axle and bears against the brass *B* underneath it, thus furnishing the necessary resistance to the action of the levers and throwing the same load upon the lower brass *B* as is imposed by the levers directly on the upper brass by transmission through the pin *D*, the latter being passed through a hole in the beam *C*. The pressure was transmitted to both the upper and the lower brass by suitable iron blocks (shown in the cut directly above and below the brasses), representing as nearly as might be the ordinary form of the top of a journal box.

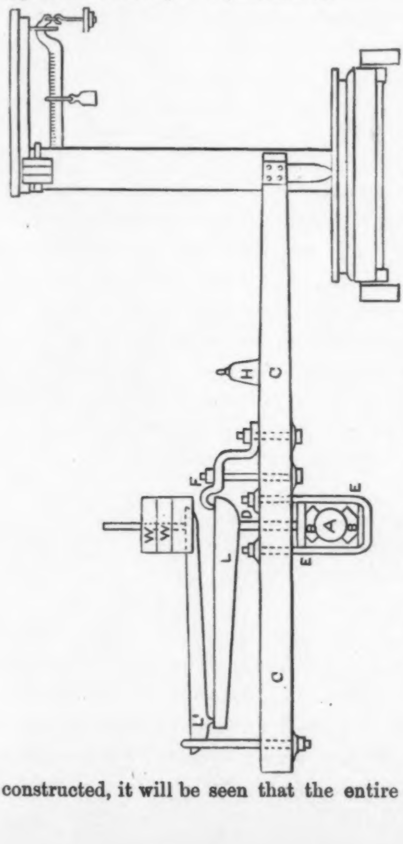


Fig. 1.—Apparatus for Testing Journal Friction in a Lathe.

As thus constructed, it will be seen that the entire apparatus (when

properly balanced, which is perfected by the light counterpoise *H*) is poised in unstable equilibrium on the axle *A*, and opposes no resistance to motion in either direction, except such as arises from friction. A very heavy load may be thrown on the bearings, viz., 6000 pounds (3000 pounds on each bearing) for every 24 pounds of load, *W* placed on the extremity of the compound lever, but the only weight thrown upon the lathe-centres is the dead weight of the apparatus itself, which was kept constant at 205 pounds.

The load thrown upon the bearings by this apparatus consists (1) of the dead weight of the entire apparatus, including both brasses, the weights *W*, and all intermediate parts; which rests entirely on the upper brass *B*; (2) the load thrown upon the pin *D* by the weight of the levers themselves, which was determined by fixing the centre of gravity of each lever *L* and L^1 by balancing it upon a knife edge, and then considering the weight of each to be concentrated at its centre of gravity; and (3) the strain produced on the levers by the addition of the weights *W*.

These last two strains, (2) and (3), produce an equal reaction against the upper brass (through the pin *D*) and the lower brass (through the yoke *E*), whereas the dead weight of the apparatus, although likewise transmitted through the levers to the pin *D*, produces no reaction against the lower brass. It is, at first sight, somewhat confusing to see how both the *dead* weight of a load *W*, and the same weight *W as multiplied by leverage*, can reach the upper brass by transmission through the same levers to the same pin *D* and brass *B*, as they plainly do, and yet in the one case produce an equal reaction against the lower brass, and in the other not; but the reason will be clear if we conceive the levers to be blocked up so as to be inoperative. The centre of gravity of the entire apparatus (after having been properly balanced by the counterpoise *H*) is directly above the pin *D*, and produces strain upon the upper brass only. By removal of the blocking the conditions as respects dead weight are in no way changed, but the additional strain caused by the leverage is added to that heretofore transmitted through the pin *D*, and this latter only produces reaction against the lower brass through the yoke *E*.

By this arrangement the pressure upon the two brasses is never exactly equal, that on the upper brass being always in excess by 205 pounds, or 8.5 pounds per sq. in. of journal section. Under the

heavier load, however, this error is so far below other causes modifying friction that there is no error of any moment in taking the entire pressure against both brasses and dividing it by 2 for the pressure on each.

When the axle A is caused to revolve, the lever C is held stationary by the platform scale, and it is obvious that the pressure produced upon the scale furnishes an exact and direct measure of the journal friction. It was found in practice that this pressure, varying from 10 to 140 pounds with the proportions actually adopted, could be weighed with as much delicacy and ease as if it were a material substance resting upon the platform of the scale. Under a given load and speed of journal the friction produced, although it did not remain absolutely stationary, varied so very little and so slowly that the beam of the scale would sometimes vibrate slowly and gently between the guards (sometimes touching the upper one and again returning to the lower, but for the most part touching neither) for 10 or 15 minutes at a time. On the other hand, when the brass was growing hot, by continuing the test for a considerable time, the friction would continue to increase so that the scale-weight had to be continually moved; but the change was never so rapid but that it could be easily followed and studied with the scale, with an absolute certainty that the friction existing for the moment was being accurately weighed. The difference in friction caused by temperature was found to be a very great one, as will appear in the summary of results, but in the absence of arrangements for accurately determining the temperature, no very close results as to its precise effect were attempted or claimed.

Considerable trouble during the tests was experienced from a tendency to heat, owing mainly, it is believed, to the crude form in which the apparatus was actually constructed; a difficulty which it would be easy to correct had opportunity favored to construct a more carefully proportioned and permanent apparatus; but, as the plan of the tests contemplated only a limited range for a single specific purpose, and also required that the conditions should be such as actually obtain in railroad service, this was not found necessary. When the bearings began to grow hot they were removed, and the effect was tried of simply cooling them with water, as in railroad service (although somewhat more carefully). On replacing them it would often be found that they would run cool for hours, during an entire series of tests, after warming up to a

temperature of 75° to 80° F, which seems to be that of minimum friction, which bearings naturally assume when in good condition, under such loads as are usual in railroad service.

As the failures in designing such apparatus are as instructive as the successes, it may be noted that the entire success of this apparatus depends upon the use of a platform scale, or some equivalent device for weighing the strains, in which the measurement of the strains is as nearly as may be absolutely statical, no motion of the bearing whatever being necessary in order to express a variation of friction. It was at first attempted to use spring scales to measure the friction, with the idea that variations of friction could be more delicately and readily read. The vibration which would almost instantly set up, seemed to indicate quick and great irregularities of friction, and absolutely forbade any useful indications from the readings. A heavy, constant weight was then added to the scale, in order to oppose by its inertia any too rapid vibrations. Checks of various kinds to restrain the vibration within limits, and also extra stiff springs, with a multiplying indicator to give delicacy of reading, were tried; but all such remedies were found absolutely useless, until the trial of the platform scale, as a last resort, furnished a complete remedy for the difficulty. Even with a platform scale, an infinitesimal motion of the entire apparatus must take place to cause the beam of the scale to vibrate, but this motion is so exceedingly minute that it at least had the effect to reduce the irregular changes to so small an amount as to indicate that the variations *then* existing would not have been sufficient to cause such violent vibrations as had previously occurred, thus indicating that the latter must furnish in the main their own cause. The well-known pendulum testing machine, designed by Prof. R. H. Thurston, from which such excellent work has been obtained, does not seem to be affected by any such difficulty, at least at the high speeds at which it is used, although it relies solely upon the motion of the bearing in order to express difference of friction; but as its indications in one important respect, which will be spoken of below, do not agree with those obtained by the writer, it is here suggested as a possible (and it is believed true) explanation of this discrepancy, that the indications of such an apparatus cannot be fully relied on at a time when rapid variations of co-efficient are known to be taking place. For it is plain that when a change of coefficient is taking place, which must not only express itself statically by registering an in-

crease of strain, but which must also *do work* by lifting a heavy weight and causing a motion of the bearings, before it can express itself statically upon the indicator, a disturbing influence is introduced which may have a serious modifying effect upon the resulting coefficient, when the latter is momentarily varying, as in stopping or starting—an effect which may not only prevent a precise expression of the coefficient which actually exists, but which might also modify the coefficient itself. That theoretical reasoning alone cannot be fully relied upon to determine whether this could or could not occur, is evident from the fact that bearings attached to a heavy pendulum, which they must lift before indicating friction, and bearings attached to a beam pressing upon a spring scale, to which a heavy weight is attached to resist erratic motions by its inertia, are, so far as the writer can perceive, under equivalent mechanical conditions ; yet experience shows that the one works thoroughly well under high and regular speeds, while the other will not.

In the apparatus shown in Fig. 1, it is plain that the lever C may be of any length, and that if it be made 2, 3 or 4 times as long as the radius of a car-wheel, measuring from centre of axle A, the resistance weighed will be $\frac{1}{2}$, $\frac{1}{3}$ or $\frac{1}{4}$ as great as would be encountered with an *axle* (2 bearings) in service bearing a similar load. The shorter the lever, the greater will be the pressure on the scale. In the apparatus in question it was made 33 inches long, so that the actual pressures weighed had to be multiplied by 2 and then divided by the total load in tons on both bearings to get the resistance in pounds per ton such as would be encountered by the locomotive acting by tension through the draw-bar.

To determine from these resistances R, the coefficient of friction C, with an axle I and wheel D inches diameter, we have :

$$C = \frac{R}{2\,000} \times \frac{D}{d} = \frac{R}{2\,000} \times \frac{33}{3.44} \text{ for a } 3\tfrac{7}{16} \text{ journal.}$$

Or, conversely, to determine the resistance in pounds per ton, having given the co-efficient, C, we have:

$$R = \frac{2\,000\ C}{\frac{D}{d}} = \frac{2\,000\ C}{9.6-} = 200\ C, \text{ approximately.}$$

It has been preferred in this paper to deal with resistances in pounds per ton, instead of the coefficient of friction, for two reasons :

1st. The determination of these resistances, and not investigations of the general laws of all friction, was the end in view in the experiments.

2d. The coefficient proper is a minute decimal, conveying no impression to the mind in itself, whereas resistances per ton are something that engineers are already familiar with, and being expressible with few digits and in integral numbers, the mind much more easily grasps and follows their relations to each other.

For the same reasons, the velocities here spoken of are miles per hour of train speed. Multiplying the velocities given by 9 gives, very approximately, the journal speed in feet per minute.

In the comparisons which follow, with various experiments the approximate formula, $R = 200\ C$ has been used to convert the recorded coefficients into pounds per ton. This is only correct when the diameter of a railroad journal is 10 times the diameter of the wheel. In general, at the present time, it ranges from less than 9 to 9.6 times, the latter having been the ratio in the present test; so that the use of the approximate formula for converting co-efficients obtained by others into pounds per ton gives a result about 4 per cent. too small. In view of the fact, however, that these results differ 300 to 400 per cent. from each other, in many cases under circumstances which seem to entitle them to equal credit, this error has not been deemed of moment, provided its existence be remembered.

The apparatus heretofore described is, when properly constructed, believed to possess every important advantage of the various testing machines in use, with some peculiarly its own. It is very light and cheap; the actual weights to be handled are very small, so that they are readily changed, and but little strain is produced on the machine; it can be used in any ordinary lathe and with an ordinary platform scale, enough varieties of which can be obtained without special construction to satisfy every requirement; it is positive in its action throughout, and no delicate computation and construction of scales is necessary for its use; and it admits of any desired delicacy of readings by the simple substitution of more delicate scales. The common platform scale of the shop where the tests were made was deemed sufficient in this instance, since the stresses actually weighed ranged so high that the error of observation from lack of delicacy in the scales could rarely exceed a fraction of one per cent.

The experience gained in making the tests indicated that the following modifications of the design shown in Fig. 1 would be expedient in constructing a machine for a more thorough and extended investigation.

The beam C should be of iron, with the yoke E and the fulcrum bearings near F and L^{I}, an integral part thereof. The pin D should be a slotted block shaped like the diagram at the side and surrounding the bar C. An adjusting screw should be provided to answer the same purpose as the nut F in Fig. 1, viz.: to take up the spring of the levers and other parts of the apparatus under varying loads W. In the apparatus as constructed, with the main beam of wood, this adjustment was a serious annoyance and required constant change with every change of load. More or less of this must be expected under any circumstances, and hence the fulcrum near F should be readily capable of adjustment by set-screw.

The weights W should be carried by a suspension yoke underneath the axle, and entirely out of the way, instead of being superimposed upon the extremity of the levers. The weights, when not in use, should be carried suspended from the yoke E, so that the dead weight of the apparatus may remain constant, avoiding troublesome corrections. A light counterpoise H will, in any case, be essential, or at least expedient to avoid unnecessary delicacy of construction. All fulcrum bearings should, of course, be knife-edged in the usual manner. Lubrication by a pad of waste or felt, or by an oil bath, if desired, is very easily provided for, by attachments unnecessary to describe. In the present series of tests a simple pad of waste on each side of the journal was used, which was kept constantly saturated with West Virginia mineral oil by a free supply. This was considered to fairly reproduce the conditions of ordinary railroad practice, and no more; as was throughout the purpose of the tests. The best scale for the purpose is the compound form with double beam, one for heavy weights and the other for minor variations.

The journal used was $3\frac{7}{16}$" x 7", giving an area of section of 24.05 square inches. The levers, brasses and axle were kindly loaned for the purposes of the test by Mr. J. Withycombe, Division Master Car-builder of the Lake Shore and Michigan Southern Railway, and were parts of an oil-testing machine designed by him, known as the "Centennial Oil-Tester," which indicates comparative co-efficients of friction only, and not their absolute value, so that it was not suitable for the purposes of these tests. The axle was set very slightly excentric, so as to imitate the effect of an imperfectly centered wheel. This probably somewhat increased the co-efficient, although very slightly at the slow speed used. The effect of

end play in distributing lubricants was imitated by the occasional use of manual force. It was found possible to do this in great degree, and it was generally found to have a slight beneficial effect upon the co-efficient, but only slight; especial pains was at all times taken to have the journal well lubricated before beginning each test. The journals and brasses were fairly well polished by use up to their average condition in service, but no more.

The tests made are shown in Table I herewith, and graphically in Fig. 2. Three different loads only were used in testing, corresponding as nearly as might be to the loads on bearings of a loaded car, empty car and truck alone. Each one of these it was designed to test a number of times at all the speeds which the lathe used admitted of, but the table will show that it did not prove possible to do this in the limited time at the writer's disposal. The tests were made on several different days and at ordinary shop temperature, and whenever a bearing heated above 150° F. the tests were suspended and the bearings cooled, since no means had been provided for accurate measure of temperature. Each test, at any given speed and load, was continued for from 5 to even 30 minutes, when the bearings were cool, in order to be certain that it was a fair average. When the bearings were hot the tests were shorter and the bearings were retained as nearly as might be at the same temperature by waiting a considerable interval between each test. During a test the resistance would generally fluctuate, slowly and gently, from 10 per cent. to sometimes 20 per cent. higher or lower than the average afterwards taken. This change was considered normal, and arose from no discernible cause. When the fluctuations were greater than this they were generally very much greater, and arose from heating of the bearings. Several series of tests were taken under such varying conditions of temperature that they were not deemed worthy of preservation. They gave results in the main intermediate to those shown in the table, which, except as noted, are believed to have been taken under approximately equal conditions as to temperature and all other disturbing causes.

TABLE I.

ABSTRACT OF TESTS MADE WITH APPARATUS SHOWN IN FIG. 1.

West Va. Mineral Oil. Free lubrication by pad.

Resistances are expressed in pounds per ton of train resistance. Divide by 200 for co-efficient of friction.

Speed of Journal.		Load on Journal. ($3\frac{7}{16}$ x 7.)										Higley Pat. Bearing.	
Revolutions per Hour.	Equivalent Speed of Car. Miles per Hour	205 lbs. = 8.5 per sq. in.	1 439 lbs. = 29 lbs. per sq. in. = 2.88 tons per Car.		7 439 lbs. = 157 lbs. per sq. in. = 14.88 tons per 8-wheel Car.				13 439 lbs. = 279 lbs. per sq. in. = 26.88 tons per 8-wheel Car.			Load 7 439 lbs. +	Load 13 439 lbs. +
			Cool.	Hot.	Cool.	Cool.	Cool.	Hot	Cool.	Warm.	Hot.		
0+	0+	....	....	24.5	21.5	23.6	22.9	24.1	{ 22.4 20.0 }	20.9	20.9	6.7	4.9
144	0.24	24.5	17.3	21.0	14.1	18.8	21.5	20.8	15.3	16.9	20.1	6.2	4.8
212	0.37	....	10.3	....	Grew hot.	14.1	Grew hot.	18.8	13.3	15.2	18.6	5.9	4.8
327	0.54	....	....	....	....	13.7	....	17.0	10.1	15.0	16.9	5.6	4.7
600	0.98	....	....	13.9	....	11.0	16.1	16.1	6.7	13.3	14.6	5.6	4.6
1 440	2 36	....	8.3	....	....	9.4	Cooled in	12.1	4.6	8.1	10.0	4.8	3.5
2 400	3.93	....	....	....	....	8.0	water.	10.5	4.1	6.1	7.4	4.0	3.1
3 600	5.89	....	....	....	....	7.0	6.7	8.0	4.1	....	5.7	....	2.7
6 000	9.82	....	....	....	. ..	6.0	....	6.7	3.8	....	5.0	3.0	2.2
7 200	11.78	....	....	....	....	5.1	....	6.0	3.6	....	....	....	....

Velocity in miles per hour × 9 = (approximately) journal speed in feet per minute.

The intensity of the strain per sq. in. of journal (longitudinal section) is indicated graphically in this (and the following) diagrams, as follows:

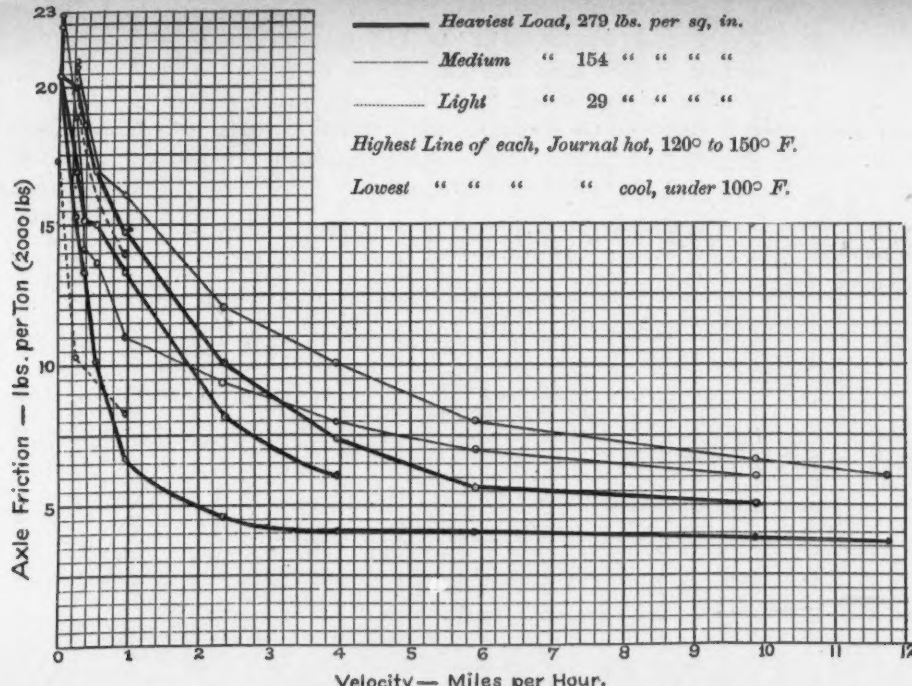


Fig. 2.—Diagram of Results of Tests as Tabulated in Table 1.

NOTE.—*In all the diagrams below, as also in Fig. 2 giving results of the writer's tests, the journal speed has been reduced to its equivalent train velocity in miles per hour and the co-efficient of friction to its equivalent in lbs. per ton tractive resistance to the locomotive.*

INTENSITY OF LOAD PER SQ. IN. INDICATED BY THICKNESS OF LINES.

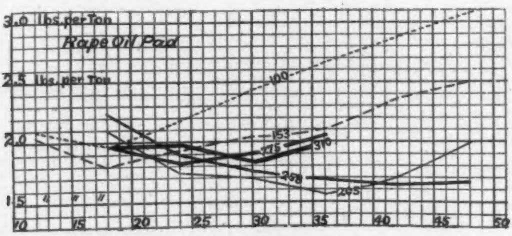
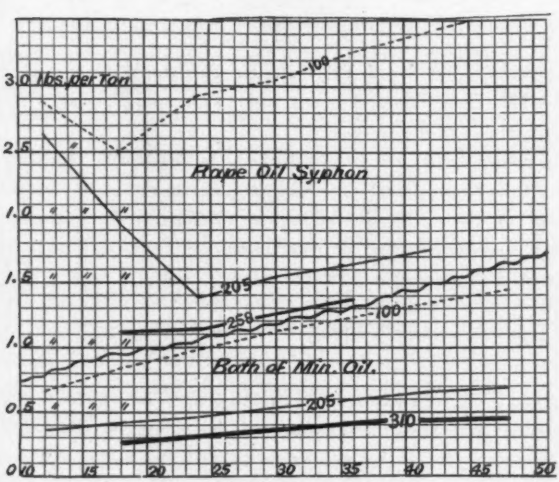


Fig. 3.



Figs. 4 and 5.

Figs. 3, 4. 5, Results of Mr. Beauchamp Tower's tests, giving effects of high velocity, variation of pressure and differences of lubrication upon co-efficient of friction.

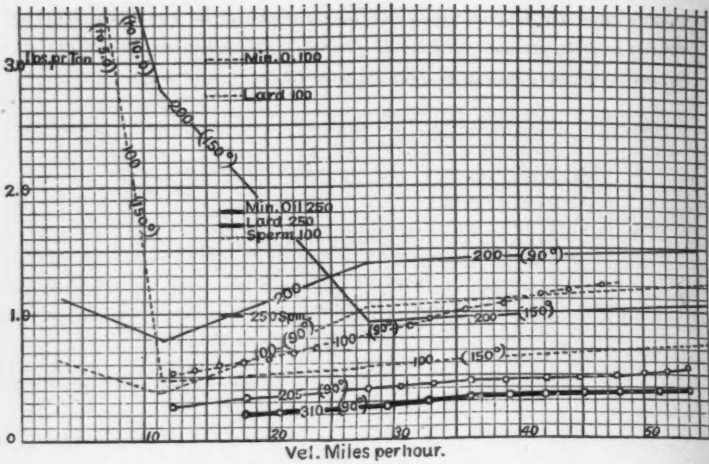


Fig. 6.

Comparative result of Prof. R. H. Thurston's tests with sperm oil and Mr. Beauchamp Tower's tests with sperm bath; the latter indicated thus—o—o—o—o—

(The most notable fact in this diagram is, that while Thurston's and Tower's tests agree almost precisely, with sperm oil, at 90° temperature and 100 lbs. per sq. in., increasing the pressure to 200 lbs. per sq. in. caused a marked *increase* of co-efficient in Thurston's tests and an equally marked *decrease* in Tower's tests.)

Deductions from the Tests.

(*Tons of* 2 000 *lbs.*)

In order to derive such insight into the general laws of friction as may be possible from these tests, they will be compared somewhat with those previously made by the writer on railroad rolling-stock by the gravity method, as elsewhere referred to; with the very complete and thorough investigations of Professor R. H. Thurston, M. Am. Soc. C. E., as set forth in his treatise on the subject,* and also with a still

* Friction and Lubrication Determinations of the Laws and Co-efficients of Friction by New Methods and with New Apparatus. By Robt. H. Thurston, M. Am. Soc. C. E., New York. The Railroad Gazette, 1879.

more complete (in some respects) series of tests recently made in England by Mr. Beauchamp Tower, under the auspices of the Institution of Mechanical Engineers.* In the apparatus for the latter tests a suspended dead load was used of the actual weight which it was desired to throw upon the bearing, instead of using springs, as in Prof. Thurston's apparatus, or leverage, as in the writer's.

The discrepancies between these two thorough and careful investigations are even more instructive than their coincidences, especially for comparison with the much less extended tests of the writer, which, as a rule, show a higher co-efficient of friction than either of them. Mr. Tower's investigations, however, did not touch at all on one of the chief ends to which the writer's tests were directed, viz.:

Initial Friction.

The writer's observations under this head were exceptionally complete, and the conclusions reached were as follows:

1. Friction at very low journal speeds of 0+ is abnormally great, and more nearly constant than any other element of friction, under varying conditions of lubrication, load and temperature. It varies from 18 to 24 pounds per ton (co-efficient, .09 to .12) for loads of from 30 to 280 pounds per square inch. Within those limits it is not greatly modified by load or temperature.

2. This abnormal increase of friction is due solely to the *velocity of revolution*, continuing unchanged so long as the velocity is unchanged and returning to the same amount whenever the velocity is reduced to the same rate, barring exceptionally slight variations, probably due to differences of lubrication and temperature. It is not appreciably affected by the fact that the journal may be just starting into motion, or is just coming to rest, or is temporarily reduced to a velocity of 0 + during continuous motion.

3. At velocities higher than 0 +, but still very low, the same general law obtains. The co-efficient falls very slowly and regularly as velocity is increased, but is constantly more and more affected by differences of lubrication, load and temperature.

* The figures used by the writer were taken from the Report in "*The Engineer*" for March 7 and 21, 1884.

4. A *very slight* excess of initial friction proper (varying from ½ pound to 2 pounds) could generally (but not always) be observed over that which continued to exist at the nearest approach to a strictly infinitesimal velocity which it was possible to obtain. This difference was, by analogy, ascribed solely to the fact that the lowest continuous velocity attainable was not strictly infinitesimal, and the final conclusion was drawn that—

5. There is no such phenomenon in journal friction as a *friction of rest*, or a *friction of quiescence*, in distinction from (*i. e.*, differing in amount from) friction of motion at slow velocities, and due to the fact of quiesence. Consequently, the use of such a term, although convenient, is scientifically inaccurate, in that it ascribes the phenomenon to the wrong cause, and to a cause which is not necessary for its existence. The fact that friction of rest, as such, *appears* to exist, is due solely to the fact that no journal or other solid body can be *instantly* set into rapid motion by any force, however great. There must be a certain appreciable instant of time during which the velocity is infinitesimal and gradually increasing.

This interesting fact, which is believed to have been here observed for the first time (no other apparatus being known to have been used suitable for determining it), was determined with great completeness by many tests. Very slow motion could be produced at any time by revolving the driving pulley of the lathe by hand when geared for a slow speed. With a little experience, the weight on the scale-beam could be placed in advance at a point which would be a trifle less than the initial friction proper, and (when properly placed) it would barely lift when motion first began, and then have to be moved back a notch or two only, to weigh the friction which continued to exist indefinitely. Similarly, when a test at comparatively high speed was about to be concluded, the scale-weight would be placed to measure the same pressure, or a little less, as existed in starting, and it was always found to indicate in stopping substantially the same friction as in starting. The same test was made by interrupting tests at speed, so as to give a continuous motion, but to suddenly reduce the speed to 0+. These tests were repeated again and again, with practically identical results.

Comparing these results with others, they agree very closely indeed with the writer's conclusions from the results of his gravity tests, as will

be seen below. The only reference to this matter in the report of the late tests by Mr. Beauchamp Tower is that "initial friction was found about twice as great as the friction in motion," indicating that the matter could not have been made a subject of investigation. No low speeds nor low pressures at all, in fact, were tested in those experiments, since they began at a journal speed corresponding to 12 miles an hour, where the writer's tests left off, and gave no pressures less than 100 pounds per square inch.

Including all other known results on this subject, we have—

"Initial" Journal Friction, (*i. e.*, at velocity of 0+).

Writer's conclusions from journal tests, above, say	19 to 25 pounds per ton.
Writer's conclusions from gravity tests of rolling stock (see Trans. Am. Soc. C. E., February, 1879), "at least"	14 to 18 " " "
Prof. R. H. Thurston ("Friction and Lubrication," page 175), W. Va. oils	22 to 28 " " "
Prof. R. H. Thurston ("Friction and Lubrication," page 175), sperm	14 to 28 " " "
Prof. R. H. Thurston ("Friction and Lubrication," page 175), lard	14 to 22 " " "
Prof. Kimball (*Am. Jour. Sci.*, March, 1878, or Fr. and L., page 186)	22 to 31 " " "
In addition, it may be noted that the writer has taken pains to observe with some care at various times that in ordinary service no railroad cars can start themselves from rest, nor can they, in general, be started without the use of much force, on a grade of .7 per cent. (= 14 pounds per ton, 36 ft. per mile), but that they will generally (but not always) start of themselves on a grade of 1.1 to 1.2 per cent. (= 22 to 24 pounds per ton, 58 to 63 ft. per mile), indicating an "initial" friction of	20 to 24 " " "

These results agree wonderfully well with each other, the averages running 18, 16, 25, 20, 18, 25½ and 22 pounds per ton, the average of all

being 18.0 to 25.0 pounds per ton, or 20½ pounds as the general average of all. This corresponds to the accelerating force of gravity on a 1 per cent. (52.8 feet per mile) grade, and that being also the lowest grade, by universal railroad experience, upon which cars can be relied on to start off from a state of rest with little or no assistance, the correctness of this co-efficient may be considered as well determined.*

But as respects the friction of journals when coming to rest, Prof. Thurston's results differ markedly from the writer's. He finds this friction, "at the instant of coming to rest," to be nearly constant instead of varying considerably with the pressure, and to be equivalent to only 5 or 6 pounds per ton, in some cases only 2 to 3 pounds per ton,† instead of 14 to 28 pounds per ton, as at the instant of starting. It seems rational that there should be this difference, since the journal is more likely to be well lubricated in coming to rest, but the writer did not find it so, and the point was tested so many times in so many different ways that he feels compelled to believe that the discrepancy arises from the theoretical deficiency in Prof. Thurston's apparatus, before alluded to, for testing *rapidly varying* and almost instantaneous changes of co-efficient. That such a change of resistance, if it be called upon to *do work*, dynamically, before it can express itself statically upon the index, cannot but introduce a possible source of error, is made still plainer if we remember that a force of this kind which was strictly instantaneous, however great, could not move the pendulum, and hence express itself upon the index, at all.

Normal Co-efficient of Journal Friction at Ordinary Operating Velocities.

Certain general facts seem to be clear from all the various tests here considered :

The first of these is that (1) the character and completeness of lubrication seems to be immensely more important than the kind of the oil, or even pressure and temperature, in affecting the coefficient.

* On a 0.7 per cent. grade (14 pounds per ton) the writer found it impossible in several instances for six men pushing, two with pinch-bars, to start two loaded box-cars into motion. In no single instance out of over sixty did cars start without some assistance. This indicates that a statement on page 14 of "Friction and Lubrication:" "The resistance in starting * * * has for its measure ⅜ of 1 per cent., or 8½ pounds per ton," requires correction; being inconsistent indeed with experimental results given in the same volume.

† "Friction and Lubrication," page 175. On page 209 it is stated that "it is nearly constant, and may be taken at .03," equivalent to 6 pounds per ton.

This is very clear from the diagrams (Figs. 2 to 6) showing the various results. Mr. Tower found that lubrication by a bath (whether barely touching the axle or almost surrounding it) was from six to ten times more effective in reducing friction than lubrication by a pad. By this method of lubrication Mr. Tower succeeded in reducing the co-efficient in a large number of tests to as low a point as .001, equivalent to only 0.2 pound per ton of tractive resistance, and the general average in the bath tests, under all varieties of load and speed, is given as only .00139. or 0.278 pounds per ton, against 1.96 to 1.95 pounds per ton with syphon-lubricator, or pad under journal. These results are very far below any heretofore reported, as will be seen from the following general average of results; not considering now the comparatively minor variations produced by ordinary working differences in temperature, load, etc.

The normal journal friction, under favorable conditions, deduced from various series of tests, may be summarized as follows for velocities greater than 10 miles per hour, or 90 feet per minute, journal speed:

Beauchamp Tower, bath of oil	.278	lbs. per ton.
" " pad or syphon	1.9	" " "
Thurston, light loads	2.75	" " "
" heavy loads	1.75	" " "
Wellington (gravity tests of cars in service)		
light loads	6.0	" " "
heavy "	3.9	" " "
" direct tests (as shown in Fig. 2)	5.1 3.7	" " " " " "
Thurston, inferior oils (Fr. and Lub., p. 173)	4.8 3.0	" " " " " "
Morin, continuous lubrication	6.0 to 10.8	" " "

These discrepancies, especially as they are accompanied by many minor ones, are very instructive, as showing that the character of lubrication is the great cause of variation of co-efficient. Thus, Thurston's experiments show almost everywhere a very marked advantage in sperm oil over all others for reducing the co-efficient. This does not appear at all in Mr. Tower's tests. Thurston also finds that with sperm as a lubricant and temperature 90° F., increasing the load from 100 to 200 pounds per square inch increases the co-efficient materially. On the other hand, Mr. Tower, who agrees almost precisely with Thurston with sperm at 90°

and 100 pounds, finds that increasing the pressure to 200 pounds materially *decreases* the co-efficient. The extent of these discrepancies are shown in Fig. 6. Other minor discrepancies of this kind might be pointed out. They are not, it is believed, to be taken as indicating a lack of either care or correctness in either experimenter, but simply as showing the overmastering effect of minute differences in the condition of the lubrication. This was also curiously shown in two ways in Tower's experiments :

1. It was accidentally discovered that with bath lubrication the bearing is actually floated on a film of oil between the lubricated surfaces, which is so truly a fluid that it will rise through a hole in the top of the bearing in a continuous stream and exert a pressure against a gauge equal to more than twice the average pressure per square inch on the bearing. This is precisely what theory would require if the lubricant were a perfect fluid.

2. Towers' apparatus required that the journal should be revolved first one way and then the other. It was found that the friction was always greater when the direction of motion was first reversed. The increase varied considerably with the newness of the journal. "Its greatest observed amount was at starting, and was almost twice the nominal friction, and it gradually diminished until the normal friction was reached, after about ten minutes' continuous running. This increase of friction was accompanied by a strong tendency to heat and seize, even under a moderate load. In the case of one brass which had worked for a considerable time it almost entirely disappeared." It is with apparent justice concluded that the phenomenon must be due to the interlocking, point to point, of the surface fibres after having been for some time stroked in one direction.*

In view of the variations of several hundred per cent., often, which are produced in the lowest co-efficients of friction by minute differences in lubrication, as shown by comparison of Thurston's and Tower's tests, and in view also of two further facts, viz. :

1. That the lubrication of railroad journals is far more imperfect than an oil bath, and rarely equal even to pad lubrication, and that the oil is rarely free from dust and of uniformly good quality; and—

* The phenomenon thus observed has an interesting bearing, it may be noticed in passing, upon a theory deduced by Dr. Charles B. Dudley, chemist of the Pennsylvania Railroad, that the fibres of steel in the top of a rail head are in reality subjected to a *bending* stress, and it lends much support to his conclusion, that such tests ought, consequently, to be an approximate measure of the probable durability.

2. That the condition of the surface of ordinary railroad journals and bearings is and necessarily must be inferior to such as are stated to have been employed in Thurston's and Tower's tests;—it seems reasonable to conclude that the writers direct tests (Table 1 and Fig. 2) correctly represent journal friction under ordinary working conditions, and that it may be taken at 5.0 to 6.0 pounds per ton with empty cars, and 3.5 to 4.0 pounds per ton with loaded cars or heavy passenger cars, at the velocity of minimum friction, which appears to be from 10 to 15 miles per hour.

These results closely correspond with the results obtained by the writer from gravity tests of cars in ordinary service; the latter results giving 0.5 to 1.0 per ton greater resistance, but including rolling friction between rail and wheel, as well as journal friction. This brings up the question of

The Proportion of Rolling Friction in Railroad Service due to Journal Friction only.

If the preceding conclusions may be accepted, the rolling friction *proper* in railroad service must be very small indeed, not exceeding 1 pound per ton. No existing experiments bearing directly upon this question are known to the writer, nor is it easy to see how such can be devised. The only references, even, to the question which can be discovered are contained in Trautwine's "Pocket Book" and Thurston's "Friction and Lubrication." Mr. Trautwine ascribes 1 pound per ton only to rolling friction. Professor Thurston, on the contrary, states that "frictional resistance on railroads is principally rolling friction," and even that "at low speeds axle and flange friction may probably be neglected."* These statements, although positive, are not stated to be based on any other authority than deductive reasoning from the low coefficients of journal friction obtained in Professor Thurston's experiments, and it is confidently believed that they err in making too little allowance for the wide variations in journal friction which result from slight differences of conditions. Certainly they are inconsistent, not only with the results of the writers tests, but also with a fact narrated in the same volume,† that the use of pure lard oil and the test of sperm for a certain time on a certain railroad had the effect to increase the

* "Friction and Lubrication," p. 13.
† Friction and Lubrication, p. 205.

number of cars hauled "about 10 per cent." It being now well determined, and universally admitted, that the total rolling friction of trains in service is only 4 to 5 pounds per ton, such a result could hardly be exceeded if the whole rolling friction, both rail and journal, were wholly abolished, instead of merely alleviating what Professor Thurston declares to be an insignificant element in the total.

It also seems proper to note in this connection that no theoretical loss whatever exists from the compression of a *perfectly elastic* substance, such as a rail may be assumed to be, and to a great extent the entire permanent way as a whole, under a rolling load. In Fig. 7, the compression at any point, whatever it may be, is proportional to ordinates from the line C C to the periphery of the wheel P. The elastic resistance is in proportion to these ordinates, and the semi-segments F F^1 represent in magnitude and position the total elastic forces operating

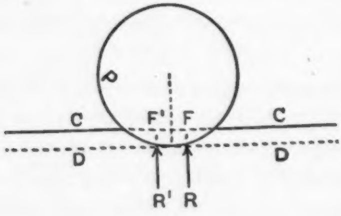


Fig. 7.

to retard and to accelerate. The resultants, R and R^1, of these parallel forces must pass through the centre of gravity of these semi-segments F and F^1, and must each be equal to *half* the total load resting on the wheel. It follows clearly from the figure that the moments of these accelerating and retarding forces are equal, so that they neutralize each other.*

The Extent of the Conversion into Heat of the Energy lost by Friction.

The tests made, under the auspices of the Institute of Mechanical Engineers, by Mr. Beauchamp Tower, gave co-efficients far lower than any

* It may be noted that it also follows from the figure that as the load upon each point on the surface of the rail is in proportion to the compression, the maximum strain which falls upon the fibres at the center is almost exactly equal to $\frac{1}{2}$ greater load than the average for the entire surface in contact.

results heretofore obtained, as before stated. An argument advanced by Mr. Tower* that co-efficients of journal friction, even in regular service, must always "in reality be more like .0035 than .035," or 0.7 pounds per ton, instead of 7 pounds, would, if sound, almost destroy the credibility of the tests here recorded, by making it doubtful if a journal friction of 4 to 6 lbs. per ton could exist for more than a few moments without melting off the axle. The argument, however, contains a possible fallacy (which it seems essential to note), in that it takes for granted that *all* the energy destroyed by friction is converted into sensible heat, and none lost in doing work against the molecular cohesion of the iron and brass, or in producing chemical or molecular changes in the lubricant, so that it does not take the form of an increase of sensible temperature. It seems probable from the very facts advanced by Mr. Tower, that only a fraction of the power lost with the ordinary co-efficients of friction can reasonably be accounted for as converted into heat; so that, if so, the argument falls to the ground by proving too much.

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* Mr. Tower's communication was as follows (From "*The Engineer*," April 4, 1884, under the heading, "WHAT IS FRICTION?):

Many people have expressed great astonishment at the extraordinary low co-efficients of friction given by the late experiments. In engineering text books the co-efficient of friction for lubricated bearings is put down at between $\frac{1}{10}$ and $\frac{1}{20}$, whereas our experiments showed that it is between $\frac{1}{300}$ and $\frac{1}{800}$, and that by the most meager lubrication possible it could not be reduced much below $\frac{1}{100}$. So that in round numbers the co-efficients given in the text books are from 10 to 30 times too high. The heat which would be generated with, say, a co-efficient of $\frac{1}{10}$, would be such as to prevent such a bearing working, except at very low speeds, as will be shown by the following calculation: Suppose a bearing running with a surface velocity of 300 feet per minute, and loaded with a pressure of 500 pounds per square inch. Suppose a co-efficient of $\frac{1}{10}$; the amount of work expended per square inch will be $50 \times 300 = 15\,000$ foot-pounds per minute $= 1166$ units of heat per hour. Now, it is said that a square foot of surface of a locomotive fire-box will evaporate a cubic foot of water per hour, which is equivalent to 416 units per square inch per hour, so that a co-efficient of friction $\frac{1}{10}$ gives 2.79 times the heat per unit of surface passing through the plates of a locomotive fire-box. A co-efficient of .03585 would, with a load of 500 pounds, and a speed of 300 feet per minute, just give 416 units per hour per square inch.

When we consider the difference of temperature which causes this rate of transference of heat in the locomotive fire-box, it seems probable that in order to obtain a similar rate of transference of heat from the bearing to the surrounding air, something like the same difference of temperature would have to exist, or, in other words, the bearing would have to be near white-hot. Indeed, looking at the matter from this point of view, it seems difficult to understand how a bearing can run with a co-efficient as high as $\frac{1}{100}$. It probably could not if it was not that the square inch which we have been considering as connected to a mass of metal exposing a great many square inches of surface to the air. The explanation of why bearings run without becoming red-hot, is to be found in the fact that the co-efficient of friction is in reality more like .0035 than .035, and even then the heat generated must be equal to the evaporation of 1.2 cubic inches of water per square inch of bearing per hour.

BEAUCHAMP TOWER,

19, Great George street, Westminster, S. W., April 2."

Mr. Tower takes the case: A pressure of 500 pounds per square inch (almost double usual railroad practice), and a journal speed of 300 feet per minute (= about 34 miles per hour, train speed), with a co-efficient of 0.1 (20 pounds per ton, train resistance), and he finds the work expended per square inch will be $50 \times 300 = 15\,000$ foot-pounds per minute, = 1 166 H. U. per hour.* It is then advanced, that as a locomotive fire-box will only evaporate about 1 cubic foot per square foot per hour, which is equivalent to 416 H. U. per square inch per hour, it follows that friction, under such a co-efficient, would generate 2.79 times as much heat as passes through the plates of a fire box; and a co-efficient of .03585, or about 7 pounds per ton, would just generate the same amount of heat as passes through the fire-box per hour. This being plainly impossible, it is therefore concluded that the co-efficient is in reality more like .0035 than .035."

Considering the force of this statement as an argument, it is beyond doubt that the difference in temperature between the two sides of a fire-box sheet is, at the very lowest calculation, 3 000° F., and generally nearer 3 500° F., and the conveyance of heat away from this comparatively thin surface is assisted by a violent and very complete circulation *of water*, which is known to be able to convey away from such a surface from 120 to 150 times as much heat as air in the same time and under the same conditions.

Therefore, granting the coefficient of journal friction to be only .0035, or 0.7 pounds per ton, the heat generated per square inch would even then, under Mr. Tower's assumption, be $\frac{1}{10}$ as much per hour as ordinarily passes through the fire-box sheets in regular service. Consequently, to dissipate this heat:

(1.) The difference between the temperature of the outside of the journal-box and inside of the brass must be $\frac{1}{10}$ as great as the inside of a fire-box; or 300° to 350°:

(2.) The brass and attached parts of the journal-box must oppose no more resistance to the passage of heat than the shell of a fire-box:

(3.) The medium for finally dissipating the heat must be as efficient as the violent circulation of water which exists inside a locomotive boiler in service.

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* Taking the mechanical equivalent of heat at 772 ft.-lbs. $\frac{15\,000 \times 60}{772} = 1166$.

Instead of these conditions obtaining, the temperature of the inside of the brass is not ordinarily much over one-third as great as 300° to 350°; the brass and journal-box oppose at least three times as much resistance to the passage of heat, and air is, at the very lowest calculation, one hundred times less efficient than water for conveying away heat, with an equally rapid circulation. Consequently, the amount of heat which can be dissipated from a journal-box is $3 \times 3 \times 100 = 900$ times less than passes through the shell of a fire-box, and, if the reasoning by which all power lost in friction is converted into heat were sound, the co-efficient of journal friction could not exceed $\frac{.0935}{900} = .00004$, or .008 pounds per ton. *Per contra*, the bearing has the advantage that the exposed area for radiating heat is several times larger than the generating surface. The exterior area of a standard journal-box (only a small portion of which, however, has direct metallic connection with the bearing) is about 500 square inches, or say 18 times the area of the brass. After all allowances are made for this compensating advantage, however, it would seem as if only a fraction of the energy lost, with the lowest co-efficients ordinarily assumed, can be accounted for as converted into sensible heat.

Resistance of Freight Trains in Starting.

It will be seen in Table I and Fig. 2 that the abnormally high co-efficient of friction at starting continues during the period of getting up speed, and thus constitutes an extra tax upon tractive power for some little distance after getting under way.

The following conclusions may, it is believed, be drawn:

1. The resistance at the beginning of motion in each journal is equal (as before stated) to about 20 pounds per ton, or say 15 pounds per ton over the average friction in motion. Except, therefore, for the "slack" which always exists in freight trains, enabling the cars to be set in motion one at a time, such trains as are usually hauled could not be started at all by the locomotive.

2. A velocity of 0.5 to 3 miles per hour, or, on an average, 2 miles per hour, must be attained before the journal friction falls to 10 pounds per ton, or 5 pounds above the average motion.

The average during this period may be taken at 12 pounds per ton.

3. At 6 miles per hour the journal friction is at least 1 pound per ton higher than at usual working speeds. The average journal friction between 2 and 6 miles per hour may be taken as at least $2\frac{1}{2}$, if not 3 pounds per ton higher than the normal.

4. During the period of getting up speed, the normal law of acceleration of velocity is so interfered with by the varying co-efficient of friction that the velocity attained at any given point may be rudely taken as directly proportional to the distance run, so that the increase of velocity would be represented graphically by a right line instead of by a parabola tangent to the horizontal line of normal velocity in motion.*

Assuming these facts, we have the following conditions in a freight train which is so heavily loaded that it may be assumed to have to run 3 340 feet, or $\frac{5}{8}$ of a mile, to acquire a velocity of 10 miles per hour:

1. The average velocity will be 5 miles per hour, and the time occupied 7.6 minutes.

2. The increased tractive force needed to accelerate velocity only will be 2 pounds per ton; since communicating that velocity is equivalent to lifting the train through 3.34 feet vertically, and $\frac{3.34}{3340} = 0.10$ per cent. grade = a resistance of 2 pounds per ton.

3. For $\frac{1}{5}$ of this distance, or 668 feet, the total demand upon the tractive power is:

2 pounds per ton for acceleration,
12 " " " " extra rolling friction.
—
14 pounds total additional traction, equal to a grade of 0.70, or 37 feet per mile.

4. For the next 1 336 feet the total demand upon the tractive power is similarly found to be 4.5 to 5 pounds per ton over the normal, equivalent to the effect of a 0.225 to 0.25 per cent. grade, or 12 to 13 feet per mile.

These grades, therefore, represent the reduction at stations or stopping places which it is essential to make to fully equalize the demands upon the tractive power of locomotives while in motion and when getting under way. The fact that such heavy reduction of grade at stations

* It may be noted that this was very nearly the case in Messrs. Galton and Westinghouse's tests of retardation by brakes. See "Pennsylvania Railroad," by James Dredge—closing plate.

may be said never to exist, while yet such heavy trains are hauled, must be due, in part, to the use of sand in starting, and in part to the fact that the full adhesion of the locomotive is not used up on the open road. To utilize to the utmost the power of locomotives, such reductions are believed to be the first thing which should be attended to in laying out a new road or in improving an old one.

Effect of Temperature on Co-efficient of Friction.

It will be noted in Table I and Fig. 2 that a high temperature exerted a very marked adverse influence upon friction at low velocities. Lack of exact notes on the temperatures reached prevents any further deduction than that, so far as they can be estimated, the results agree very closely with Prof. Thurston's formula that the co-efficient increases as the square of the increase of heat over 90° to 100° F. at speeds under 12 miles per hour. No tests were made above that speed to confirm Prof. Thurston's deduction that at higher speeds the law changes. Mr. Tower's tests were nearly all made at a constant temperature of 90°, and the effect of temperature does not seem to have been made a subject of investigation.

Effect of Load per Square Inch of Bearing on Co-efficient of Friction.

Comparison of the results obtained by the writer, and by Messrs. Thurston and Tower and others, as shown in Figs. 2, 3, 4, 5 and 6, develop this curious fact : that while the results differ quite widely in fact by several hundred per cent. in what may be called the typical or average co-efficient of friction, they all agree quite closely in finding that the effect of increased load, within working limits, is to very materially diminish the co-efficient. Mr. Tower, in fact, goes so far as to state, as one of the results of his tests, that it almost seemed at times as if it was approximately true that the *absolute* loss by friction was entirely independent of load, the co-efficient falling almost to half when the load was doubled. But it seems plain, from the diagrams given herewith, that this result is only true on account of the unprecedentedly low co-efficients which he obtained by his very perfect lubrication. Inspection of the diagrams will show that the general law of variation from increase of load is not materially different in the different tests, despite the wide variations in the average co-efficients.

Effect of Velocity Over Twelve Miles per Hour.

Figs. 2, 3, 4, 5 and 6, taken in connection, seem to show the following:

1. The velocity of lowest journal friction is 10 to 15 miles per hour.

2. With bath or other very perfect lubrication, there is a very slight increase of journal friction accompanying velocities up to 55 miles per hour (Figs. 5 and 6).

3. With less perfect lubrication, as with pad or syphon, greater velocity is as apt to decrease as to increase the co-efficient (Figs. 3, 4 and 6). The latter being more like the ordinary lubrication in railroad service, we may say, without sensible error, that the co-efficient of journal friction is approximately constant for velocities of 15 to 50 miles per hour.

This has been the assumption which all investigators of railroad friction, to date, have been compelled to make, and it is, in some respects, fortunate that it proves not far from true.

Higley Roller-Journal Bearings.

This apparatus is shown in Fig. 8, and was also shown in Fig. 7 of the writer's paper before referred to, giving a report of his gravity tests (see page 37, Trans. Am. Soc. C. E., 1879). The results deduced from the direct tests herein described, as shown in Table I, as compared with the gravity tests are as follows:

	Journal Friction (pounds per ton) at velocity of		
	0 +	3 to 5 M. per Hour.	10 to 20 do.
By direct tests, full load..	4.9	3.1 @ 3.5	2.7 @ 2.2
" gravity " " " ..	4.0	3.3	2.8
By direct tests, light load..	6.7	4.8 @ 4.0	3.0
" gravity " " " ..	5.0	5.0	4.1

The correspondence between these tests, made as they were by such different methods, is thought to be very close and satisfactory. These later tests confirmed exactly the correctness of the writer's previously stated conclusions, that the Higley bearing was nearly as efficient as theory would indicate in reducing initial friction, but loses nearly all of this advantage under speed.

The Higley bearing was then, and is now, in very extensive use on street cars, and was in use on several railroad cars at the time of the

writer's tests. Mr. Higley's dissatisfaction with the results of the gravity tests was a principal reason why these later tests were undertaken, which fully satisfied that gentleman's doubts, as results could be seen with his own eyes, as weighed upon a scale, without having to accept the indirect (although, in fact, equally positive and valid) indications of computations based on variations of velocity of rolling stock.

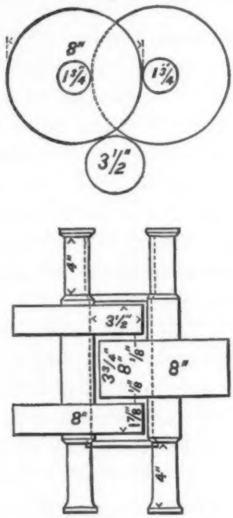

Fig. 8.

DISCUSSION.

ROBERT H. THURSTON, M. Am. Soc. C. E.—I find the paper of Mr. Wellington very interesting and very instructive, and regret very much that time and opportunity do not offer for as careful a study and as complete an examination of the facts and conclusions embodied therein as I should be glad to make.

The apparatus described seems to me to be both ingenious and trustworthy, and I have no doubt that the work done with it is accurate and valuable. The behavior of the scale-beam during its operation is the best evidence that the machine is very accurate and amply sensitive. The experience of its designer, in the earliest stages of his work, and which drove him to use the platform scale, is one which I can fully appreciate, having had a similar experience. I have for many years used the platform scale in connection with the Prony brake for the same reason as is here given—the annoying fluctuations and vibrations which are inseparable from the use of a spring, and the inconvenience of working with weights in a scale-pan. I find that my own form of machine for testing lubricants possesses the same advantage as is here noted, in consequence of the inertia of the pendulum. This I find to be a decided advantage, so much so that, although some of the earlier designs of this machine included the use of a spring, I have never adopted them for my own use or for the market.

It is true that minute and sudden changes of friction are not indicated, as is observed by Mr. Wellington; but, on the whole, I do not find this fact a disadvantage. The occasion for noting such fleeting quantities seldom, if ever, arises, and the steadiness of the machine for ordinary work is an advantage of such vastly greater moment that I should never think of sacrificing the latter to the former. The platform scale possesses the same advantages and the same disadvantages, although I am not prepared to say in the same degree. Whatever either gains in the one direction it loses in the other. On the whole, I am inclined to think that the simplicity of the pendulum apparatus and its exactness, arising from the fact that there is absolutely nothing between the point at which measures are desired and that at which the measures are read, to affect accuracy, is an advantage which could hardly be compensated by the introduction of mechanism to give any new effect. A large number of these machines are now in use, many of them under the direction of members of the

Society, I think, and testimony on these points will probably be easily secured. I do not think that the discrepancies between the results obtained by Mr. Wellington and other observers are due to differences of apparatus; I am very sure that they are usually either due to differences in methods of working or of conditions, noted or unnoticed, or to the mistake which nearly all investigators find themselves very liable to make—that of drawing general conclusions from a too limited collection of facts. In some cases the difference may arise from the error, on the part of the investigator, of supposing that the apparent and seemingly obvious conditions are the real conditions. This is especially liable to be the source of error in such experiments as those on friction of lubricated surfaces, in which work it is impossible to be at all times sure that the journal is in the same state at any two consecutive observations. I think that a comparison would show that the same virtues and the same faults will be found in the two forms of apparatus which are here referred to, and, although probably in different degree, still to such an extent that we must look elsewhere for the cause of any apparent difference of testimony as to general laws controlling friction of lubricated journals.

I am pleased to see that the effect of end play of the journal was observed. I have not been able to see that it makes any practically important difference in friction, so long as the lubrication is satisfactory; but I have found it a very useful action in the prevention of scoring of the surfaces, the cutting of the journal and bearing, and in the production of a good distribution of the oil when the lubrication is not very free.

Examining Mr. Wellington's conclusions, I find that he has been able to confirm very completely a view of the method of variation of friction with speed, as the surfaces come to rest, which has, I think, been already more than suspected by earlier experimenters.* I think that there can now be no doubt that the friction of motion, at low speed, gradually increases as the velocity of the rubbing is decreased, until it becomes a maximum at the moment of coming to rest, and that the friction of motion and the friction of rest are a "continuous function." Other things being equal, it follows that the value of the co-efficient is a function of the velocity of rubbing. I see no reason for dispensing with the term "friction of rest" or "friction of quiescence," however. The term has its uses, and there is no objection, so far as I can see, to retain-

*As by M. Bochet, Comptes Rendus, April, 1858; by Professors Ewing and Jenkins, Proc. Roy. Soc., 1876; Ency. Britt., Article Friction; and by the writer, Friction and Lubrication, § 70.

ing it, especially as it is becoming well understood that the facts cited by Mr. Wellington are good foundation for his conclusion. In fact, there is a true "friction of rest;" it is the quantity which measures the adhesion of a heavy body to the surface upon which it remains at rest. Its value can be determined on any one of the many machines known to me, and, I presume, it in all cases represents a limit to which, under the given conditions, the value of the friction of motion approximates as the velocity of rubbing decreases. The law of variation of the co-efficient can also be very exactly determined on any of these machines by obtaining the measure of the co-efficient at different speeds, decreasing in velocity down to as near zero as may be convenient, and then plotting the results and ascertaining the nature of the curve so obtained.

I am inclined to think that the value obtained by Mr. Wellington is more nearly correct—certainly for the conditions of his work—than those obtained by me, and think it more than possible that his interpretation of the cause of the differences noted is correct. This is a matter that I have never investigated as I would like.

A difference is pointed out between the results obtained by Mr. Tower and by myself. This difference is partly due to difference in the methods adopted, that of Mr. Tower being in some respects different from the system in common use, which I endeavored as perfectly as possible to follow, but also to the fact that the figures quoted from my book represent variations due, not to pressure alone, but to alterations of temperature as well, which latter changes brought the critical point at which the journal begins to get dry probably down to a pressure between one and two hundred pounds per square inch. A better set of figures is to be found at page 177, and others at pages 173, 174 of my "Friction and Lubrication." I think that it will be found that the co-efficient always decreases with increase of pressure, and at low pressures very rapidly, provided that the lubrication is kept thoroughly effective. Any change, however slight, in other conditions will be found to make an apparent modification of the law. The quotation from Tower represents the fact better than the quotation from my work. Table I, here given, represents the results of experiments recently made on the same journal as was used for the work described at page 177 of "Friction and Lubrication," a half dozen years earlier. The difference for a range of pressure from 100 to 267 pounds per square inch is there shown, and also some effect of change of temperature:

TABLE I.

Record of Tests of Lubricants—Sperm and Lard Oils.

Mechanical Laboratory, Department of Engineering, Stevens Institute of Technology.

Composition—	No. of test	1A	2A	1B	2B
(1.) Sperm Oil	Pressure on journal, lbs. per. sq. inch	267	267	100	100
(2.) Lard Oil	Total pressure on journal, lbs.	800	800	300	300
Investigation—	Amount of oil used on journal, m. g.	Full	and	free	supply.
To determine effect of pressure.	Average co-efficient of friction	0.0053	0.010	0.009	0.013
	Minimum " " "	0.004	0.004	0.008	0.016
	No. of revolutions per minute	1 000	1 000	1 000	1 000
	No. of feet traveled by rubbing surface per minute	330	330	330	330
	Elevation of temperature, max. Fahr.	45°	70°	30°	50°

Time. Minutes.	Temperature.	Co-efficient of Friction.	Time. Minutes.	Temperature.	Co-efficient of Friction.	Time. Minutes.	Temperature.	Co-efficient of Friction.	Time. Minutes.	Temperature.	Co-efficient of Friction.
1A			2A			1B			2B		
0	90	0.007	0	85	0.021	0	90	0.012	0	82	0.022
5	105	0.006	5	105	0.014	5	100	0.009	5	110	0.017
10	120	0.006	10	130	0.013	10	110	0.008	10	125	0.015
15	130	0.005	15	150	0.007	15	112	0.008	15	127	0.014
20	130	0.005	20	155	0.006	20	115	0.008	20	130	0.013
25	135	0.004	25	155	0.004	25	117	0.008	25	131	0.013
30	135	0.004	30	155	0.004	30	120	0.008	30	132	0.013
	Average	0.0053		Average,	0.010		Average,	0.009		Average,	0.016

Record of Tests of Lubricants—Mixed, Mineral and Animal.

Mechanical Laboratory, Department of Engineering, Stevens Institute of Technology.

Composition—	No. of test	I.	II.
Heavy petroleum, 80 &	Pressure on journal, lbs. per sq. inch.	100	267
Lard, 20	Total pressure on journal, lbs	300	800
Investigation—	Amount of oil used on journal, m. g	Full and	free supply.
To determine effect of pressure.	Average co-efficient of friction	0.0147	0.0073
	Minimum " " "	0.0103	0.0059
	Revolutions per minute	1 200	1 200
	No. of feet traveled by rubbing surface per minute	400	400
	Elevation of temperature, max. Fahr	60°	70°

Time. Minutes.	Temperature.	Co-efficient of Friction.	Time, Minutes.	Temperature.	Co-efficient of Friction.
I. 0	85	0.0333	II. 0	90	0.0093
5	130	0.0147	5	115	0.0081
10	135	0.0127	10	135	0.0081
15	140	0.0107	15	145	0.00[illegible]3
20	145	0.0107	20	150	0.0059
25	145	0.0103	25	155	0.0063
30	145	0.0103	30	160	0.0069
	Average	0.0147		Average	0.0073

TABLE II.

Record of Tests of Lubricants—Mineral Oil.

Mechanical Laboratory, Department of Engineering, Stevens Institute of Technology.

Composition—	No. of test	1	2	3
(1.) Heavy black petroleum.	Pressure on journal, lbs. per sq. inch.	267	267	267
(2.) Sperm oil.	Total pressure on journal, lbs	800	800	800
(3.) Lard oil.	Amount of oil used on journal, m. g.	Full and	free	supply.
nvestigation—	Average co-efficient of friction	0.014	0.0053	0.010
To determine effect of temperature.	Minimum " " "	0.010	0.004	0.004
	No. of revolutions per minute	1 000	1 000	1 000
	No. of feet traveled by rubbing surface per minute	330	330	330
	Elevation of temperature. max, Fahr.	90°	45°	70°

Time. Minutes.	Temperature	Co-efficient of Friction.	Time. Minutes.	Temperature.	Co-efficient of Friction.	Time. Minutes.	Temperature.	Co-efficient of Friction.
0	85°	0.016	0	90	0.007	0	85	0.021
5	135	0.013	5	105	0.006	5	105	0.014
10	160	0.010	10	120	0.006	10	130	0.013
15	165	0.015	15	130	0.005	15	160	0.007
20	170	0.015	20	133	0.005	20	155	0.006
25	175	0.015	25	135	0.004	25	155	0.004
30	175	0.015	30	135	0.004	30	155	0.004
	Average..	0.014		Average..	0.0053		Average..	0.010

Record of Tests of Lubricants—Mixed, Mineral and Animal Oil.

Mechanical Laboratory, Department of Engineering, Stevens Institute of Technology.

		Rise.	Fall.	Rise.	Fall.
Composition—	No. of test	....	....	....	....
Heavy petroleum, 80	Pressure on journal, lbs. per sq. inch	100	100	100	100
Lard. 20.	Total pressure on journal, lbs...	300	300	300	300
Investigation—	Amount of oil used on journal, m. g.	Full	and	free	supply.
To determine effect of heat.	Average co-efficient of friction....	0.018	0.007	0.0154	0.008
	Minimum " " "	0.004	0.005	0.005	0.006
	Total No. of revolutions	....	....	....	....
	Total No. of feet traveled by rubbing surface	....	....	....	....
	Elevation of temperature, max., Fahr.	270°	120°	220°	120°

Temperature—Rise.	Co-efficient of Friction.	Temperature—Fall.	Co-efficient of Friction.	Temperature—Rise.	Co-efficient of Friction.	Temperature—Fall.	Co-efficient of Friction.
70		340	0.005	110	0.043	340	0.006
120	0.067	330	0.005	120	0.040	330	0.006
130	0.063	320	0.005	130	0.040	320	0.006
140	0.057	310	0.006	140	0.040	310	0.006
150	0.043	300	0.006	150	0.035	300	0.006
160	0.030	290	0.006	160	0.025	290	0.007
170	0.027	280	0.007	170	0.020	280	0.007
180	0.020	270	0.007	180	0.016	270	0.008
190	0.027	280	0.007	190	0.015	260	0.008
200	0.013	270	0.007	200	0.012	250	0.010
210	0.010	260	0.008	210	0.011	240	0.010
220	0.008	250	0.008	220	0.009	230	0.011
230	0.006	240	0.009	230	0.007	220	0.012

Tempera-ture—Rise.	Co-efficient of Friction.	Tempera-ture—Fall.	Co-efficient of Friction.	Tempera-ture—Rise.	Co-efficient of Friction.	Tempera-ture—Fall.	Co-efficient of Friction.
240	0.005	230	0.010	240	0.006	210	0.013
250	0.005	220	0.012	250	0.006		
260	0.005	210	0.012	260	0.006	Average...	0.008
270	0.005			270	0.006		
280	0.005	Average...	0.007	280	0.005		
290	0.004			290	0.005		
300	0.004			300	0.005		
310	0.004			310	0.005		
320	0.005			320	0.005		
330	0.005			330	0.005		
340	0.005			340	0.006		
Average.	0.018			Average...	0.015		

The same differences are seen to be given in Table II.

In passing, it may be remarked that the curious and important effect of flooding the journal with oil by running it in a bath has been explained, at the last meetings of the British and the American Associations (Montreal and Philadelphia), in a very ingenious and interesting manner, by Professor Reynolds, who ascribes this phenomenon to the action of fluid friction in causing a static head at the crown of the journal by the conversion of dynamic head, when the flow of the oil with the journal surface is checked by contraction of the channel as the two surfaces approach the line of bearing.

Mr. Wellington, discussing the proportion of friction due to journal friction only, in railway work, misapprehends the statement made at page 205, "Friction and Lubrication," to which he refers. The correct statement is that "the experiment was recently tried, on one of the great railroads of this country, of using pure lard oil in summer and the best of sperm oil in winter, on freight trains, employing a man to attend simply to lubrication. The effect was to increase the number of cars which could be hauled by each engine about ten per cent., and to secure much greater regularity of service."

This statement was not intended to bear upon the assertion that the

axle and flange friction can be neglected. The first statement is one of fact, and the second is one of deduction, which could only be correct under the assumption that the conditions of axle friction were the best possible. I am not sure that it is even then true in general, and am perfectly sure, as is every railway engineer, that it cannot be true of the average train in average condition of journal. The saving made as above stated was due to the better preservation of journals, the more careful selection of lubricants, and the promptness with which defective journals were removed.

I agree with Mr. Wellington perfectly in what he has to say here as to the real value of the resistance. The load which an engine can draw at low speed on a level track is that which it can start from rest. With the system of loose coupling this is a limit which is far higher than with the system of rigid coupling in use on our passenger trains.

It is a pleasure to find such accordance as is said to exist between the experiments of Mr. Wellington and my own in the determination of the effect of variation of temperature. Of the nature of the general effect there is now, I think, no doubt. The extent of this variation is subject to modification by circumstances, which will probably bear further investigation.

Table II contains figures recently obtained, and which may serve to supplement those which I have previously published. A difference is always observed between the figures obtained during the rise and during the fall of temperature. This is due partly, if not principally and usually, to the fact that the thermometer takes up the heat at a somewhat different rate from the journal surface. The figures taken during the fall of temperature are probably those which are to be adopted, except in cases in which the temperature has been so high as to affect the surface of the journal and bearing or to alter the lubricant. These tables also illustrate the reduction of the co-efficient of friction with increase of pressure, and the fact commented upon by the writer of the paper. The discrepancies thought to exist between the experiments of Mr. Tower and those made by me have really no existence. In the figures given at page 188 of "Friction and Lubrication," referred to by Mr. Wellington, the speed was so low that the limit of good effect seems to have fallen under 200 pounds. At higher speeds this limit rises far above that pressure. The results there given can only be interpreted for the peculiar conditions there existing. Since the date of their pub-

lication we have learned more definitely what are the effects of such variations of condition.

I shall take the earliest opportunity to study the details of the paper here discussed, and shall, I have no doubt, have occasion to reassert the opinion which I have formed already in regard to its value, and to consider the facts presented as among the most interesting and valuable that have yet been published on this very important subject. In the next edition of "Friction and Lubrication," now nearly ready for the press, I shall endeavor to profit both by Mr. Wellington's criticisms and by his suggestions, as well as by the new information which he has contributed.

W. S. Auchincloss, M. Am. Soc. C. E.—Mr. Wellington's apparatus might be so constructed that the constant weight, 205 pounds, also the applied weights, *WW*, could be counterpoised, and the problem thus reduced to simply that of weighing the statical effect of friction upon the platform scale.

If the top brass, *B*, is designed to represent the brass on a railway car axle, and the lower brass, *B*, one on the opposite end of same axle, the frictional results will be *approximately* correct for an entire axle, but can never be taken as absolutely the equivalent of what occurs in practice, for the lubrication of the lower brass will always be more perfect than on the upper brass, also the rocking of the brasses at high velocities will yield different gripping effects, which, being exerted on one and the same journal, will render it more liable to heat than it now is in daily service, with a single brass on the one journal.

There are very many features and conditions of the railway axle-journal, brass and box, which are expressed with difficulty in a testing machine, but the device of Mr. Wellington will give results that are close approximations to those that occur in railway service, and will, doubtless, yield valuable data when revised and experimented with in an exhaustive way, taking into consideration every velocity, load, proportion of parts, and variety of lubricant and mode of lubrication.

D. J. Whittemore, President Am. Soc. C. E.—The experimental results contained in this interesting paper showing decreased journal friction under increased velocity of revolution should receive the earnest attention of railway managers, as showing at what speed of train, economy of journal friction is attained.

Considering time, power, motion and matter in the abstract, I think it will be conceded that to produce instant motion throughout all the particles of a mass, however small, requires infinite force. Even eliminating inertia from the problem, I very much doubt whether the absolute resistance of journal friction in initial and infinitely slow velocity of matter in contact can be determined with close approximation by the ingenious appliance used in these experiments.

All motion generated through human appliances appears first as a series of shocks, as in steam entering the ports of an engine, transmitted through its connected parts, not perfectly diffused by any balancing arrangement, thence through shafting more or less elastic, perhaps through vibrating belts, and thence to the journal under test, and for these reasons may be considered as revolving through the force of almost infinitesimal shocks of impact. Motion generated by the agency of the human hand is far from uniform. The action of the heart itself gives impact to stress exerted by the hand. For these reasons, I am under the conviction that the frictional resistance from a state of rest to that of infinitesimal motion is underrated by the writer. Friction implies motion; therefore, I quite agree with the writer that there is no such resistance as friction of rest, but I can conceive that there is a resistance to initial movement for which the term friction of rest is not proper.

To my mind, the appliance used by the writer is admirably adapted for ascertaining the average frictional resistance of journals under varying speeds of revolution, the real object desired. In fact, it may be termed an averaging as well as weighing appliance, the difficulty experienced in the attempt to use spring balances on the first lever being so fully overcome by passing the successive shocks of motion through the leverage system of the platform scale.

With so much to commend in the endeavor, purpose and ability displayed in conducting these experiments, it is with reluctance that I feel it just and proper to take issue with the writer in the statement, "that no theoretical loss whatever exists from the compression of a perfectly elastic substance such as a rail may be assumed to be, and to a great extent the entire permanent way as a whole under a rolling load." Under a static load this is true, and this static condition is a demonstration of its truth, but under motion of the rolling body and the resulting forces brought into play, it is evident to me that the elastic curve in

front or in the direction of the movement is of different form and of greater resistance to the rolling body than that in the rear. The form that the surface of water takes in front and to the rear of swiftly moving vessels illustrates in an aggregated degree the idea I wish to convey, and it is clear to my mind that what is called rolling friction, a misnomer for rolling resistance, cannot be a constant quantity for all velocities, but is of varying value, dependent on and increasing in some ratio to the velocity and weight of the rolling body.

Wilson Crosby, M. Am. Soc. C. E.—If, in Mr. Wellington's apparatus, we consider, as he does, in his explanation, that the levers are blocked up, and that then the lower brass be brought up so as just to touch the under side of the journal, but to receive no pressure from it, then, on the blocking being removed, the condition of affairs supposed by him will exist, that is, the sum of the pressures from both brasses on the journal will be nearly twice that coming from the upper brass.

Both of these pressures operate to cause friction.

Supposing all parts of the machine to be strong enough, it is evident that the yoke may be set up to such a degree of tightness that the brasses will clasp the journal so closely as to render it impossible for it to be turned.

In this case the pressure coming on the upper brass through the pin *D* would be no measure whatever of the journal pressure.

On the other hand, if the lower brass is left slack—does not touch the under side of the journal (corresponding to the case of car journals)—the total pressure is that coming on the upper brass.

It is not stated exactly how Mr. Wellington's apparatus was adjusted in this respect, nor, certainly, what he took as his pressure giving the friction, but it appears as though he took the half sum of the pressures from the two brasses.

If this be so, in thus considering only half the pressure instead of the whole, as he should, did he not get his co-efficient of friction twice as large as it should be?

A. M. Wellington, M. Am. Soc. C. E.—The possible error suggested by Mr. Crosby is no doubt something which might easily occur, only, fortunately, it did not. It was intended to make this perfectly clear in several places in the paper, where the pressure on both brasses is spoken

of, and notably in the headings to Table I, where both the total pressure and the pressure per square inch is given in full, requiring that both brasses should be considered to make the two correspond. The computation of leverage is not affected by the use of two instead of one brass. I may also note that Mr. Crosby's suggestion that the lower brass might be left slack, confining the total pressure to the upper one, is not a possible one. To have the apparatus operative at all, the pressure on the two brasses must be equal, except for the trifling difference of the dead weight of the apparatus.

Henry R. Towne, M. Am. Soc. C. E.—These experiments were obviously directed chiefly to the determination of questions relating to railroad practice, and as such are somewhat removed from general discussion. The subject of journal friction, however, is one of wide interest and great importance, so that all engineers are interested in anything which contributes to a better understanding of it.

The most novel and interesting point developed by Mr. Wellington is in regard to what has heretofore been designated as the friction of rest, as distinguished from the friction of motion. Accepting his tests, it seems proven that there is no discernible difference between the friction in starting and the friction of very slow motion, his tests indicating the same co-efficient with a given speed, whether that speed was reached by starting from a state of rest or slowing down from a more rapid motion. This fact has direct application to many classes of machines in which the speeds of shafts are very slow, as, for instance, in hoisting machines operated by hand. It has been customary heretofore to assume a uniform co-efficient of friction for all ordinary speeds in estimating the useful effect of a given piece of mechanism, but in view of Mr. Wellington's tests it would seem that the co-efficient should be varied with reference to the speed of rotation in the case of machines in which any of the revolving parts move very slowly.

The statement that "the character and completeness of lubrication" are more important than the kind of lubricant, or even than the pressure and temperature, will meet with general acceptance. The curious results obtained by Mr. Beauchamp Tower in his recent tests have called attention to the importance of a point heretofore too often looked upon as trivial, viz.: the location and shape of the oil ways on the journal. The experiments both of Mr. Tower and Mr. Wellington emphasize the

fact that thorough lubrication is the all-important factor in determining the co-efficient of friction of journals ; that an oil bath gives the best possible result, and that, where this is not possible, pad lubrication is the next best. Excluding these, the various remaining ways require to be selected according to the circumstances of each case. The result to be sought, however, is the same in all cases, namely, a lubrication so thorough as to insure the floating of the journal on a film of oil. Where this is thoroughly accomplished the co-efficient of friction then becomes that of the lubricant rather than that of the two metals forming the journal and its bearing.

This latter question is so important, and we are at present so much in the dark concerning many of the important facts involved, that it is greatly to be desired that further investigations should be undertaken. If the Society could in some manner assume the conduct of such experiments, giving direction to them while in progress, and tabulating and publishing their results when completed, it would subserve one of the most useful ends for which it can exist, and I suggest that this question should be considered in connection with the discussions arising from Mr. Wellington's paper.

Charles Paine, Past-President Am. Soc. C. E.—This is a valuable paper, and with most of Mr. Wellington's deductions, from the experiments by himself and others, I am inclined to agree; hoping, however, for a more complete series of experiments to confirm them. The cost of making such is not considerable, and would be more than regained, I believe, by any of our great railroad companies which might undertake them, in the more certain knowledge which would be obtained of the performance to be relied upon in locomotives, and of the effect of an increase in the loading of cars. We are quite in the dark, as yet, as to what increase of friction has been caused by raising the loads upon cars from 10 to 25 tons.

Upon the Lake Shore and Michigan Southern Railway an increase in the maximum load from 10 to 15 tons per car, and of 4 tons in the average load actually carried, was not discovered by the locomotive drivers ; on the contrary, much effort being made by the division superintendents to get the utmost performance out of their locomotives, the number of cars hauled by each engine was increased at the same time as the loads. This experience, with others, and the experiments given in

this paper, make me think that the usual manner of estimating and stating the values of friction in pounds per ton is a misleading one, because the increase in load has so little effect in increasing the friction. Mr. Tower's remark, quoted in the paper before us, "that it almost seemed at times as if it was approximately true that the absolute loss by friction was entirely independent of load," may be taken to confirm this view, and is one which for other reasons deserves the attention of railroad engineers.

Some experiments of Mr. Dudley with his dynamometer car confirm Mr. Wellington's conclusions as to the great importance of lubrication as affecting train resistances. I may mention also, as confirming the views of this paper, that his experiments with stock trains upon the Erie Division of the Lake Shore and Michigan Southern Railway, where the grades do not exceed 13 feet per mile, showed that the economical speed (so far as the demand upon the power of the locomotive was concerned) is about 18 miles per hour.

In considering the "resistance of freight trains at starting," Mr. Wellington's remarks are in favor of the present "slack" in freight trains, which may not be justified by experiment; at least we do not know how much, or rather how little, slack is necessary to permit each car to be started separately. It was once believed that the Miller and the Jauncy buffing platforms would prevent the starting of long passenger trains; yet the elasticity of the springs in the couplings allows of sufficient slack for all practical purposes. In Europe there is no such thing as a slack coupling on railway trains.

I presume that Mr. Wellington would himself confine the reductions in grade at stations, to which he refers, provided they would add to the cost of construction, to termini of divisions or other main stopping places; for a railway which is completely equipped for the cheapest carriage of freight will not stop its heavy trains except to change engines. Ramsbottom's trough was designed, I believe, for saving the time of express passenger trains; yet it is much more important to save the cost of the stops of heavy freight trains. The presence of shunting engines at large stations, which can be employed to assist in starting trains at little additional expense, would also enter into the calculation, when the cost of any modification of grade is considered.

F. M. Wilder, M. Am. Soc. C. E.—I am pleased with this machine for testing the friction, and think, following the general idea, attachments

can be devised for getting the velocity, pressure and temperature graphically. As far as I have read and thought of the figures presented, and the conclusions drawn from them, I find they agree somewhat with conclusions which I came to from experiments made with the dynamometer, although I think the initial friction is too high for thoroughly lubricated car journals, as also the friction at speeds of 10 to 15 miles per hour. I have not had time to compare my figures, but I found, in substance, that the dynamometer showed on a level straight track, at a speed of 15 miles per hour, the total resistance through a distance of 2 to 3 miles was 3.25 pounds per ton, as calculated carefully from a card which was made automatically, and with a train weighing 750 tons. Of course, the element of instrumental error for the machinery would be less with such large forces than with smaller ones.

The resistance in this case should be divided into journal and wheel and flange friction, and also atmospheric resistance; and I am satisfied that this would bring the journal resistance down to about 2.5 pounds per ton with 300 pounds per square inch of journal surface. I shall hope to find leisure to discuss the paper further, with which I am much pleased so far as I have examined it.

Mr. John W. Cloud.—The great dearth of positive data on the subject of friction with lubrication, together with the consequent, or allied, primitiveness of existing methods of lubrication, renders all new light upon the subject very valuable. The question is, however, so much one of conditions that the value of any new results depends as much upon the fidelity and completeness with which the conditions are given as it does upon the accuracy and completeness of the recorded results. There is already published enough partial information to completely befog an already mystified engineer, and it behooves him to examine all new data with care, and to accept only that, all the conditions of which are explicitly stated as being correct under such circumstances, and to be skeptical of any general deductions based on existing fabrics of information.

In studying the question of "friction at low velocities," and especially of starting or "initial friction," Mr. Wellington has done well to begin by making an improved testing machine, and he has employed a form of construction which is better for this purpose than any I have before seen. The pendulum machine of Prof. Thurston is, per-

haps, as good for low velocities when constant, but to measure the friction at the moments of starting and stopping, which the author calls "initial friction," is clearly beyond the capabilities of the pendulum machine, while it is perfectly feasible with Mr. Wellington's type. I cannot but regard the former as a more convenient form of machine, however, for showing the correct friction at constant velocities, whether high or low, under any given conditions.

The main point is to have the conditions such that they can be given so as to be understood, and here I think both these machines are at fault for any valuable scientific work, or for any further use than comparisons, such as tests of oils, etc., because they both require two bearings to fit the same journal.

It may, perhaps, be safely assumed that over ninety-nine (99) per cent. of the mileage of car journals is made with the journals and bearings in good order, and any one who has had extended experience with oil-testing machines, and with railway rolling stock, knows that "good order" in service means much better surfaces than can be had or maintained by wearing them in a testing machine where the bearings are frequently removed from the journals and dust has an opportunity to enter.

It follows that "good order" in service is a much more definite statement of condition than can be otherwise had, and I think nothing would be quite as satisfactory as axles drawn from service in good order, and placed in the testing machine, with the bearings which had been worn to the journals in service, and if the results are to be applied to the resistance of railway trains as now run, the journal box should be in place with its sponging during the test, or an equivalent well be placed under the journal to fulfill the same conditions. It may be possible to wear two bearings to the same journal, successively, in service, and then remove the axle and use it in Mr. Wellington's machine with both bearings (if proper care be taken in oiling), and get results which would be more reliable than anything now on record, but this would not be quite as satisfactory because of the difference in the conditions of the bearings and in the lubrication.

There is nothing in Mr. Wellington's paper to show that this method was followed, and, if it was not, I think the resistances per ton may readily be fifty (50) per cent. higher than they should be to represent average surfaces on account of the condition of the surfaces, and upon

the supposition that the lubrication of each bearing was equal to that of service, the latter condition being rather difficult to fulfill with pads, when the journal has bearings on opposite sides.

On account of the extreme uncertainties of other resistances in railway trains besides journal friction, I cannot see that the results of total resistances of cars can be adduced to justify the accuracy and representativeness of journal frictional resistances as determined on this machine; in fact, I have frequently found with dynamometer car a total train resistance, in still weather, of four (4) pounds per ton with loaded cars and five and one-half ($5\frac{1}{2}$) pounds per ton with empty cars on level tangents at ten (10) miles per hour, which includes journal, track and air resistances, and I am, therefore, skeptical of the high figures given by Mr. Wellington as journal frictional resistances alone, as representing average service; but they may, doubtless, be readily had with slightly inferior bearing surfaces or states of lubrication. Further, in many hundreds of comparative oil tests on a Thurston machine, each test lasting for one hour, with $3\frac{1}{4}$" x 7" journal and a total pressure on both bearings of 8 680 pounds, and a speed equivalent to fifteen (15) miles per hour with thirty-three (33) inch wheel, with a temperature of bearings maintained at 100° F., and with two-thirds ($\frac{2}{3}$) cu. cm. of mineral oil applied to the journal at the commencement of the test and no addition afterwards, with a narrow strip of canton flannel lamp-wick let into the lower bearing to act as a redistributor of oil and to catch the worn particles, we have obtained an average co-efficient of friction, for the hour, mostly ranging between 0.0050 and 0.0060, equivalent to about one (1) pound to one and one-quarter ($1\frac{1}{4}$) pounds per ton resistance. But then it must be added that the journal is of hardened steel, which doubtless reduces the friction, and no care has been spared to make good bearing surfaces and to keep them in order. They do not, however, fit their journals as well as do good bearings in service. It is quite impossible to make any direct comparison of such tests with Mr. Wellington's results, but they certainly would offer some grounds for believing his figures to be too high.

In regard to starting, or initial friction, I have never made any experiments on an oil-testing machine, as it is not within the range of the Thurston machine to do so; but it is well known that starting friction is far in excess of subsequent frictional resistance, and I regard the extent of this excess in railway service to be dependent upon many things,

such as kind of oil, weather, time of rest and condition of surfaces, and their various combinations; and, in regard to the author's deductions from the tests under the head of "initial friction," would object to his drawing the final conclusion, No. 5, that "there is no such phenomenon "in journal friction as a friction of rest, in distinction from friction of "motion." I understand a force to be that which produces, or tends to produce, to retard or to change the direction of motion, and that friction is a force whose tendency is to retard motion under certain circumstances, and believe it is as scientifically correct to speak of a "friction of rest" as it is to speak of the resistance to a compressive force of a pole which supports a weight against the action of gravity; and if there is no friction of rest I ask: what is the force that prevents a car starting—by the action of gravity—down a grade less than 52.8 feet per mile, since the resistance to rolling on the rails is so very small?

This "friction of rest," doubtless, becomes the same as the "initial friction" at the instant of starting; but it may be anything between that and zero while standing, as it is possible for any force not capable of producing motion to be acting to produce it, and the force of friction simply counteracts it, and no more. The author seems to have lost sight of the fact that forces may be acting and counteracting without any resultant motion.

In regard to "the extent of the conversion into heat of the energy lost by friction," the author seems to be very far out. The indestructibility of matter, the continuity of motion, and the persistence of energy in some form or other, have been sufficiently well established. But here we have a proposition amounting to a positive annihilation of energy; for any "work done against the molecular cohesion of the iron and brass," or "in producing molecular changes in the lubricant," would appear afterwards in some form, or be annihilated. It does not appear in the fact of attrition or in molecular changes in themselves, unless the oil is volatilized, because the iron and brass or the lubricant have no more inherent energy, at the same temperature, in the changed state, than they had before—in fact they have less, in proportion to the work done upon them, and an equivalent amount of heat or other form of energy has appeared in its stead. It is possible that a certain portion may take the form of electricity, but it must be very small; at least we have employed very delicate means to detect it, and have discovered no electric current. In regard to chemical changes in the lubricant, I can

see no reason for supposing such to take place, unless the lubricant contains something which will attack the iron or brass at the temperatures existing, in which case it is in so far an improper lubricant. Further, I understand it to be a pretty general principle, and perhaps universal, that chemical changes are accompanied by the liberation of heat rather than by its absorption; so that there seem to be very sufficient reasons, since we can find no appreciable electric current, for considering that an amount of heat equivalent to the work done in overcoming friction must appear and must take the form of sensible temperature, in good service, as there is nothing to be melted or volatilized which could cause it to disappear and become latent heat.

To follow Mr. Wellington still further in this matter, he says: "It seems probable from the very facts advanced by Mr. Tower "that only a fraction of the power lost with the lowest co-efficient "of friction can reasonably be accounted for as converted into heat; "so that, if so, the argument falls to the ground by proving too "much." Then follows Mr. Tower's case, which is put in such a way as to be rather indefinite in meaning and capable of two or three constructions, there being no distinction made between the number of square inches of bearing surfaces in contact and the number of square inches of radiating surface or of metallic section to conduct the heat away. I will, therefore, not attempt to discuss Mr. Tower's case, but offer the following to show that there is no difficulty in accounting for the dispersion into the air of the heat generated on the supposition that it is to be an equivalent of the work performed in overcoming the journal friction.

In order that the matter be not complicated by grades and curves, we will suppose a level tangent and a total resistance per ton of 6 pounds, at a speed of 15 miles per hour, and a train weight above the journals of 2 000 tons. We will further suppose, what we have reason to believe to be more nearly the case, that one-half of this resistance only is due to journal friction. Taking, then, Mr. Wellington's diameters of journals and wheels and his approximate ratios, and assuming that our 2 000 tons is carried on 800 journals (100 cars), we have on each axle (2 journals) a weight of five (5) tons or $3 \times 5 = 15$ pounds resistance per axle. As fifteen (15) miles per hour is equivalent to 1 320 feet per minute, we have $1\,320 \times 15 = 19\,800$ foot pounds converted into heat per axle per minute, equal to $\frac{19\,800}{772} = 25\frac{1}{2}$ heat units per minute; or, as the

specific heat of iron is about $\frac{1}{9}$, there is sufficient heat to raise the temperature of 230 pounds of iron 1 degree F. per minute. The wheels and axle, to say nothing of the journal box and bearing, will weigh at least 1 400 pounds, and will require $\frac{1\,400}{230} = 6 +$ minutes to raise the average temperature of the mass 1 degree F., if there is no loss by dispersion. The temperature is, doubtless, raised more at the journal; but it must be remembered that iron is a good conductor of heat, and that it will be rapidly conducted away as the temperature of the journal rises. Further, we have found that the temperature of journals in good order, immediately after a long run, is from 15 degrees to 20 degrees F. above that of the air. By the time such increase of temperature is reached at the journal it may easily be understood that the whole mass of 1 400 to 1 500 pounds is sufficiently warmed up to act as a very efficient means for dispersing the heat into the air through which it is rapidly moving as the heat is generated.

Further, unless electric currents are generated, or something is melted or volatilized, it seems certain that an equivalent of the 6 pounds per ton total resistance is finally dispersed into the air as sensible heat; otherwise it is difficult to see where it will appear.

The air resistance warms the air directly, and perhaps the cars. The rolling friction warms the wheels and the rails, thus giving an additional amount of heat for the former to disperse, which they are doubtless capable of doing.

If the surmise is correct, that Mr. Wellington has obtained frictional resistance considerably higher than average results in service, due to inferior conditions of bearing surfaces and lubrication, it will, probably, apply with especial force to the figures he has used under "Resistances of Freight Trains in Starting," because the "initial" friction is probably affected more in proportion with inferior surfaces than friction at speed. There is no doubt, however, that the existence of slack and elasticity in draft springs enables a locomotive to start a greater load than it would be otherwise able to do. There is one very essential point, however, which Mr. Wellington does not recognize, at least not to its full extent, as he has not considered it in determining the reductions of grade which "it is believed to be the first thing to be attended to in laying out a new road or improving an old one," viz., the fact that a locomotive can exert at least twice the pull at starting that it

can at a speed of about twenty (20) miles per hour, due to the fact that there is enough time to exhaust a cylinder full of steam without causing appreciable back pressure, whereas, at the higher speed, the admission of steam must be suppressed early and the exhaust must open early to get the maximum pull possible at that speed. The weight on drivers must, of course, be sufficient to enable the engine to exert the maximum pull at starting, and yet there is not such a material excess of adhesive weight for the pull at speed, partly on account of less time for new surfaces to interlock, and sometimes largely on account of the variations in pressure of drivers upon the rails due to the vertical excess of counterbalance. In fact, I have known engines to slip at speed which would exert twice the pull at starting on an equally good rail without slipping.

It is certainly desirable to make use of this extra power available in starting trains rather than to make such extensive grade reductions as Mr. Wellington believes should be made, especially when we consider how many heavy trains run by stations where other trains stop, and would be obliged to mount the unnecessary grades.

Mr. Beauchamp Tower.—I will first deal with the references which the author has made to the experiments conducted by me for the Institution of Mechanical Engineers.

The reason why my experiments show a lower co-efficient of friction generally than Professor Thurston's, is that my experiments were mostly conducted with oil bath lubrication, which is probably the most perfect possible method of lubrication, and consequently gives the lowest possible co-efficients. The reason for adopting this method of lubrication is stated at the beginning of my report.

With regard to initial friction, the author is, I think, mistaken when he says that I found initial friction about twice as great as friction in motion. I made no experiments on initial friction. I think that he must have misunderstood my statement that the result of reversing the motion of the journal was for a time to double the normal friction (top of page 635), and supposed that I referred to initial friction, which I did not.

The author is also mistaken in saying that the superior lubricating power of sperm oil does not appear in the results of my experiments; I must refer him to Table XI and the note underneath it.

I must also refer the author to Table IX to show that the effect of

variation of friction with temperature was a subject of observation. It will be observed that friction diminished with an increase of temperature. It must be borne in mind that this was with oil-bath lubrication, which means a perfect oil film between the journal and brass, and it was doubtless due to the oil becoming more fluid and less viscous with the higher temperature.

With regard to the author's remarks on my letter to the *Engineer*, of April 4th, stating my belief that the co-efficient of friction in practice was more like .0035 than .035, and giving reasons for the opinion based on the rate of generation of heat and the temperature necessary to carry off that heat to the surrounding air:—

I should first say that when I expressed that opinion I had in my mind a large engine, such as marine engine, bearings, in which such speeds and pressures are not only often attained, but frequently exceeded. These are bearings in which the direction of the force is rapidly alternating, and the oil given an opportunity to get between the surfaces, while the duration of the force in one direction is not sufficient to allow time for the oil film to be squeezed out. The opinion which I hold that, owing to greater facilities for perfect lubrication, the friction of a bearing under reciprocating force is considerably less than that under a constant load in one direction, is supported by the fact that it is found in practice that a bearing under the former condition will work satisfactorily under a very much heavier load per square inch than under the latter.

For instance, 250 pounds per square inch cannot be much exceeded with safety on a railroad axle, but 1 000 pounds per square inch is not uncommon on a crank pin; so that when I say that I think the co-efficient of friction is about $\frac{1}{300}$ on the crank-pin, I may quite consistently hold that it is very likely about $\frac{1}{100}$ in a railway axle.

Now, with regard to the question of the rate of generation of heat and my illustration of the rate of the passage of heat through the sheets of a locomotive fire-box, the author says that I take for granted, without proof, that all the energy destroyed by friction is converted into heat. I can only say that my warrant for so doing are the celebrated experiments of Dr. Joule, which proved that energy destroyed by friction, of no matter what kind or between what materials, was converted into heat at the rate of 772 foot pounds per unit, and upon which the whole modern science of thermo-dynamics is founded, and to prove my view to be a

fallacy he must first prove Joule's experiments and the thermo-dynamic theory to be fallacious.

The author holds that my illustration of the rate of the passage of heat through the sheets of a locomotive fire-box proves so much too much that it disproves itself. I think, however, that he has overdone the proof a good deal in his calculation to show this. He assumes that the only barrier to the passage of the heat between the fuel on one side of the sheet, at say, 3 340° and the water on the other side, at say 340°, is the want of power of conduction of the metal of the sheet, that is to say. the temperature of one side of the sheet is the same as that of the fire, and that of the other side the same as that of the water, no difference of temperature being necessary to cause the required quantity of heat to pass from the fuel to the sheet on one side, or from the sheet to the water on the other, but the whole of the 3 000° being devoted to causing the heat to pass through some half inch of copper or iron. Were this view correct the quantity of water evaporated by a fire-box would be directly in proportion to the conductivity of the metal and inversely in proportion to the thickness of the plates; that is to say, that a copper box would evaporate about twice as much as an iron one per square foot per hour, and one made of $\frac{1}{4}$-inch plates twice as much as one made of $\frac{1}{2}$-inch plates. We know that this is not the case. We also know that no part of the metal of the box is ever at a temperature of 3 000°, or anything approaching it, from the fact of the metal remaining in a solid state.

Indeed, from the fact of the metal retaining its strength and not bulging between the stays, there is very good reason for believing that no part of it has ever been more than 600° or 700°. Thus a difference of temperature of about 2 700° has been necessary to cause the heat to pass from the fuel to the sheet. Supposing this passage of heat is due only to contact of hot air, and we neglect radiation altogether, and take the author's figures, showing that the passage of heat between a solid and water is 150 times as quick as that between a solid and air. We have seen that 2 700 difference was necessary to cause the transfer of the heat from the air to the sheet, and we may, therefore, assume that $\frac{1}{150}$ of this, or 18°, will be necessary to cause the transmission of the same quantity of heat from the sheet to the water. We thus have fuel at 3 340°, one side of the sheet at 700°, the other side at 358°, and the water at 340°. The difference between the temperatures of the two sides of the sheet is thus 342°, or only about $\frac{1}{10}$ of that on which the author's calculations are based.

Of course, these figures can have no pretension to exact accuracy, but I feel sure that they represent the state of things much more nearly than the author's calculation. There is also the fact, which I pointed out in my letter to the *Engineer*, that each square inch of bearing is in metallic connection with a great many square inches of surface in contact with the air, which facilitates the conduction of the heat to the air. But still, with all these deductions, the difficulty is still sufficiently great to require a very low co-efficient of friction to explain it.

My own idea is that the trouble caused by the tendency to heat referred to by the author was caused only by his working with an excessive co-efficient of friction, or, in other words, an insufficient lubrication.

I think it probable that the rolling friction being greater than is supposed will account for the actual tractive force required for railway vehicles.

No doubt, if the metal of the wheel tires and rails were perfectly elastic and not strained at the point of contact beyond their limit of elasticity, and also if the rails themselves rested on a perfectly elastic or perfectly rigid bed, that the rolling friction would be nothing. But, as a fact, rails and wheel tires wear out, which is, I think, a proof that some of the metal is strained beyond its elastic limit and crushed whenever a wheel rolls over a rail. The imperfect elasticity of the road-bed is also another cause of resistance. Every time a train passes, the sleepers or cross-ties are pressed down further into the ballast than they rise up again after the train has passed; thus the train is always running against a slight gradient due to this cause. It seems to me that it would be very important and desirable to obtain a separate determination of rolling resistance of railway vehicles. It would be easy to do this by attaching tires on to a solid cylinder of cast-iron of the same diameter as the wheel to be represented, and about 6 feet long; the tires being put on to the proper gauge, the cylinder could be run on a railroad and its rolling resistance determined. It would thus be easy to get a load of 5 or 6 tons, if required, on the equivalent of a pair of wheels without any axle friction at all.

A. M. Wellington, M. Am. Soc. C. E.—Certain questions of great interest are raised in the discussions which have been presented, on which I desire to add a word further. I will, therefore, pass over as briefly as possible certain points, which are of minor and merely personal im-

portance, not affecting the subject itself appreciably, but simply my handling of it. Mr. Cloud's interpretation of my statement that there was no such phenomenon in journal friction as a "friction of rest, *as distinct from a friction of motion*," into a denial that no friction exists between bodies at rest tending to resist motion, was probably an inadvertence, since it does entire violence to my language in the immediate context. What I did assert was simply that experiment showed that there was no difference in the nature or quantity or co-efficient of journal friction at rest or under very slow continuous motion, but that *both* were very large as compared with journal friction at working speeds. This fact, and the fact that the state of lubrication affected the co efficient so slightly at O + speeds was thought to be new.

Mr. Cloud also appears to have overlooked my statement that the bearings and axles were stated to have been loaned from an oil-testing machine in use on the Lake Shore and Michigan Southern Railway, and were "fairly well polished by use up to their average condition in service and no more." The condition of the bearings was thus not left in doubt, although the wording of the sentence perhaps gives a somewhat too favorable view of their actual condition. Though the eye could detect no difference from the ordinary fine polish of a journal and the bearings had been much used, they were probably *not* as finely fitted to each other as even an average journal in service.

In reference to Mr. Tower's discussion, the need for certain of his minor corrections in respect to the differences between his co-efficients and Prof. Thurston's, the supposed statements in his report as to initial friction, the comparative superiority of sperm oil over others, and the effect of temperature on friction, may have arisen in part from the fact that his full report was not accessible to me, but only the abstract which appeared in the *Engineer* of March 7–21, 1884, and in part to some mutual misapprehensions, both on my part and on his, of the meaning intended to be conveyed. As these differences are in no way radical, and as their cause and the extent to which they are in fact corrections will be obvious to those caring to investigate, more detailed discussion of them seems unnecessary.

Mr. Tower's statement that his expression of belief that actual co-efficients of friction were nearer to .0035 (or 0.7 pounds per ton) than to 0.035 (or 7 pounds per ton), had reference to reciprocating bearings of large marine and other engines rather than to railroad practice, also

removes a part of the difficulty which I found in it. From the use of the illustration of a locomotive fire-box a contrary inference seemed natural.

I may again state that my conclusions given as to the co-efficient of initial friction were mainly based upon a long series of tests of rolling stock in actual service, and not upon laboratory tests with the apparatus here described, although the two happened to nearly coincide. I do not understand Mr. Cloud to dispute that ordinary railroad cars will not start of themselves on a grade of less than 1 per cent. or 20 pounds per short ton resistance. The existence of slack in long freight trains, enabling the cars to be started, as it were in detail, is, of course, a very important element in handling them, or the trains now handled could probably not be handled at all.

The suggestion, offered by Mr. Cloud, that a freight engine has an advantage in starting, which I did not fully consider, in that it has time enough to use a cylinder full of steam, I do not understand to be an important one, since cylinder power is rarely the weak point of a freight engine, but rather adhesion. A well-designed freight engine is supposed to have cylinder power enough to slip its drivers (in other words, use up its full adhesion), as a matter of course, and it generally does so liberally.

That there is any measurable excess of adhesion in starting a train, over that existing at 15 or 20 miles per hour, and especially any such difference as "at least twice as much at starting," independent of the use of sand, as Mr. Cloud states, is something which I have never heretofore known to be asserted. All the information and experimental data now known to me as on record, including notably Capt. Douglas Galton's and Mr. Geo. Westinghouse's exhaustive and elaborate experiments on brake friction, seem to positively contradict Mr. Cloud's first reason for this statement, that the co-efficient of friction between rail and wheel is greater at slow speeds "on account of less time for new surfaces to interlock," * and as to the effect of the centrifugal force

* Capt, Galton's exact language is: "The amount of frictional resistance which determines the point at which the rotation of wheels is checked varies, it is true, in the different experiments. The ratio which it bears to the weight upon the braked wheels" varies from .19 to .35, averaging .25. "But it (the variations) clearly represents simply the adhesion between the wheel and the rail, and varies only with this, and not with the speed. Thus at 60 miles per hour, diagram 15, the amount of frictional resistance which checked the rotation of the wheels was about 2 000 pounds, exhibiting an adhesion of about .191 per cent.; at 15 miles per hour, diagram 14, 2 160 pounds, or .196 per cent. As these two values are so nearly equivalent, it would appear that the effort is much the same at all speeds."

"The Pennsylvania Railroad Company," by James Dredge: Appendix on Brake Trials.

of the counter-weights; a freight train laboring between stations on its maximum grade (the only important case to consider), does not usually generate any very serious amount of centrifugal force in the drivers. A hasty computation indicates about 180 pounds per wheel at 10 miles per hour. As respects high-speed passenger trains, a careful test by the late Prof. Charles A. Smith, M. Am. Soc. C. E., of the run of a fast passenger train on a distance of 110 miles, readings being taken at every mile and 10 miles, showed no slipping of the wheels whatever.* This test was, I think, by far the most careful which has been heretofore made and publicly described in the United States, and would seem, on its face, to be decisive as respects ordinary American practice, although "imperceptible slip," in unequalized European locomotives at speeds above 60 miles per hour, has been apparently proven, and, perhaps, also in American engines at such speeds. Mr. Cloud's extended experience and great facilities for the making of accurate physical tests entitle any assertion from him to great respect; yet a statement that he has known engines to "slip at speed which will exert twice the pull in starting on an equally good rail, without slipping," is so difficult to reconcile with the other authorities quoted, that any experimental proof of it would be both interesting and novel.

I might further add, moreover, that the reductions of grade at stations suggested by me were *not* intended to be made at the cost of an increase of grade between stations, which, I think, is rarely, if ever, necessary, a little care and trifling expense generally sufficing. I have, myself, found no difficulty in so doing on several continuous ascents of 3 000 to 8 000 vertical feet, and cannot believe the difficulty could ever be serious in dealing with the lower elevations to be surmounted east of the Rocky Mountains. In general, it is rather a convenience.

In respect to the more important question of the extent to which the energy lost by friction may take other forms than an increase of sensible temperature, in which my suggestions have been controverted by Mr. Tower and Mr. Cloud, as the subject was merely incidentally connected with the purpose of my paper, and as I cannot and did not claim to have any original experimental data on the subject to contribute, an elaborate discussion of the question—although I feel the

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* The record of this test will be found in detail in the *Railroad Gazette*, 1878, p. 293

difficulty of fully accounting for the large amount of heat that must be dissipated is by no means fully cleared up by the discussions mentioned —would lead to too long an excursion from the subject now under discussion. I shall, therefore, content myself with suggesting some of the consequences which flow from admitting that all this energy is dissipated by radiation as sensible heat. I feel justified in protesting, however, that a suggestion that part of this energy may be dissipated in producing chemical or molecular changes in the lubricant or metallic substances was in no respect intended to be, nor is it fairly open to the charge of being, a denial or neglect of the well-established theories of heat and motion. It must be merely by inadvertence that Mr. Tower appears to assert that Dr. Joule's experiments necessarily require that *all* energy lost by friction shall be converted into sensible heat. If used to vaporize water, for instance, only 20 per cent. becomes so measurable. Generation of electricity is another of the possible sources of loss, into which I did not attempt to go into detail. Mr. Cloud's discussion seems to disprove the existence of the latter, and it is not probable that there is any perceptible vaporization of the lubricants. The gradual chemical change which takes place in a lubricant appears to be generally admitted to be a form of oxidation, which would liberate more heat rather than absorb it; but since every chemical process is capable of reversion, I do not understand Mr. Cloud's statement to be entirely defensible, that "it is a pretty general principle, and, perhaps, universal, that chemical processes are accompanied by the liberation of heat rather than its absorption." Not being a chemist, however, I will not further discuss it.

It should be noted here, however, as indicating the difficulty which surrounds the subject, that, according to the best existing experiments, those of M. Peclet,* the rate of transmission of heat through iron from water to water, with perfectly clean surfaces, and with efficient circulation of water, is 225 H. U. per square foot per hour per inch of metal, for each degree of difference of temperature in the water, or 0.13 H. U. per square inch per hour; any dulling or incrusting of the surface having a marked injurious effect. This is considerably over the observed rate of transmission of heat through locomotive fire-boxes, which is about 416 H. U. per square inch per hour, corresponding to a difference of

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* See D. K. Clark's "Manual for Mechanical Engineers," p. 460.

temperature of 3 200°, with 1-inch plates, or say 1 600° for $\frac{1}{4}$-inch to $\frac{5}{16}$-inch plates, assuming their efficiency for transmitting heat to be reduced one-half by dulled surfaces. That the temperature of the inner surface of the sheets is much below this, and especially so low as 700°, as suggested by Mr. Tower, will hardly seem reasonable; nor is it necessary to assume that it is from considerations of injury to or bulging of the plates, as a brief computation, based on the admitted effects of temperature on iron, would show; nor is the argument advanced by Mr. Tower of the almost equal efficiency of copper and steel fire-boxes so forcible as appears at first sight, since well-established and independent experiments show that copper and iron differ but slightly* in their efficiency for transmitting heat in this manner and under these conditions. As iron at 500° F. is some 15 per cent. stronger than when cold, Mr. Tower's assumed maximum temperature would leave the sheets considerably stronger than with no fire in the fire-box.

Now, at 1$\frac{3}{4}$ pounds per ton journal friction, with a bearing of 24 square inches sectional area, with the common load of 300 pounds per square inch of bearing and a train speed of 55 miles per hour, or a journal speed of say 500 feet per minute, *one-fourth* as much heat per square inch is generated as passes through a square inch of a locomotive fire-box in any given time. This heat, neglecting for the moment what is conveyed away by the oil or directly radiated from the bearing to the side of the journal-box, must all be conveyed away (1) through a bearing and wedge, or key, of about 24 square inches sectional area, and 2 inches thick, or (2) through an axle of but 18 square inches section. Air is admitted to be at least 100 times less efficient than water for conveying away heat, and before the heat has traveled a distance to expose 100 times as much cooling surface as the heating area of the brass, it must travel an average of at least 5 inches through solid metal. We have then, so far as I can see, the following conditions:

1. A difference between the temperature of the air and water on each side of the locomotive fire-box of 3 000°, at least, and in the journal-box only 15° to 20°, to which latter, however, something should be added for the higher temperature of the oil than the bearing surface, say, following Mr. Tower's figures for water, 18°, the total difference of temperature then becomes only $\frac{1}{100}$ part as large.

* "$\frac{1}{20}$ to $\frac{1}{30}$," D. K. Clark's "Manual," p. 461.

2. We have an average of 5 inches of solid metal in the journal-box, instead of $\frac{1}{4}$ inch in the fire-box, to pass the heat through before it reaches an equally efficient dissipating surface. This alone would reduce the heat transmitted to $\frac{1}{20}$ part of what passes through the fire-box, except for the fact that the available area of section is increasing as the heat passes farther away from the bearing. The net difference, consequently, is no doubt much less, say $\frac{1}{4}$ or $\frac{1}{5}$, instead of $\frac{1}{20}$.

These two causes together would reduce the quantity of heat transmitted to $\frac{1}{400}$ or $\frac{1}{500}$ part, whereas, as a matter of fact, we have with fast and heavy passenger trains nearly *one-fourth* as much heat passing away from the bearing as passes in an equal time through an equal area of the fire-box sheets, or 106 H. U. per square inch per hour as against 416 H. U. per square inch per hour. Mr. Cloud's illustration of a slow freight train is equivalent to 63.6 H. U. per square inch per hour. At 45 miles per hour, instead of 15, it would be three times as great. It is, of course, easy to include the whole wheel and axle as radiating surface, and so show that all the heat can disappear if it once reaches it, but it hardly seems proper to do so.

For the above reasons, it seems to me impossible to account for such large dissipation of energy without assuming either that a portion of it does not take the form of sensible heat, or that the average co-efficient of service is very much lower than generally supposed, perhaps under 1 pound per ton.

The general approval expressed of the design of the apparatus confirms my belief that it possesses special advantages for solution of a number of important and still unsettled questions. Mr. Auchincloss' suggestion that the dead weight of the apparatus might be neutralized by a counterpoise, thus reducing the pressure on the two brasses to absolute equality, is a good one. It could hardly be a counterpoise in the ordinary sense, but an adjustable spring balance, or weighted cord passing over pulleys, arranged in any convenient way to give an upward movement equal to the weight of the apparatus, would accomplish the end. The disadvantage of using two brasses to a bearing is undoubted, but it can, and should be, in part done away with by facilities to throw upon them a strong blast of air, preferably of compressed air, which would produce low temperature by expansion. With these conveniences, and the application of an ice trough to the middle of the axle, any desired temperature might probably be obtained and maintained.

measured by a pair of thermometers inserted in each end of the brass. There can be no rocking of brasses, as one gentleman has suggested, nor any motion whatever of any part of the apparatus while in use, except the axle itself. A complete oil bath for both bearings is easily arranged.

As respects the proportion of rolling friction proper, between rail and wheel, I apprehend that I may have estimated it too small, as Mr. Cloud suggests, and that it may in reality be 1 or 2 pounds per ton, or even more. While hesitating to differ from our respected President, I am unable to see how any *theoretical* loss of energy whatever can arise from the elastic yielding of the track, returning as it does sensibly to the same place. Such slight loss as appears to actually exist is easily accounted for by the loss from attrition, gradual crushing of the ties, and gradual displacement of the ballast. The plan for directly testing rolling friction, suggested by Mr. Tower, by using a solid cylinder of cast iron provided with tires, had also occurred to myself, and might work very well if a pair were connected together by an ordinary truck frame, but otherwise would hardly do, as a single pair of wheels will not work at all well on track on account of its tendency to skew. If connected in this way by bearings, the co-efficient of friction is so much higher with light loads that troublesome corrections are again necessary.

It would be fortunate if some of the still unsettled and possibly highly important questions connected with journal friction could be investigated, by a committee of this Society or otherwise, as Messrs. Towne, Paine and Wilder have suggested. The usual difficulty will probably prevent, that those who could and would do it cannot command the time or incur the expense. The apparatus here described can be so cheaply fitted up in any shop as to be peculiarly well, I think, adapted to this purpose, and may perhaps lead some one to attempt such. For making them, in addition to what has been previously suggested, I should deem the following important details:

1. The axles and brasses to be experimented on should be drawn from actual service, after fitting a *pair* of bearings to each journal, so as to be known to be as perfectly fitted as in actual service. Different qualities might well be used on different journals.

2. The platform scale should be fitted with automatic movement for the poise, such as can be furnished by several makers, connected, preferably, to a record paper moved by clock-work. The velocity of rotation is readily determined in advance for each speed of the lathe.

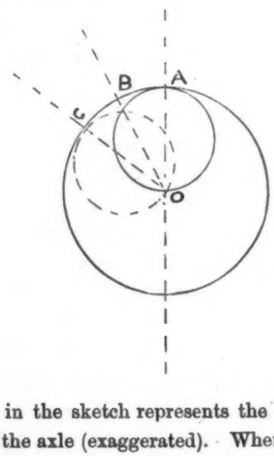
3. Very careful arrangements, by the use of ice and compressed air, should be made for obtaining any desired temperature in the bearing from zero up.

4. Facilities for momentarily relieving the bearings from pressure should be provided. Otherwise the initial friction cannot in general be overcome with the low power of the high-speed gearing, the belt slipping instead and causing annoyance.

In addition to confirming or refuting the pretty wide range of tests which were covered by the limited series I was able to make—and which I should be the last to claim were adequate to settle them positively—important subjects for tests would be the extent to which friction might be modified in service by more complete lubrication, approaching as closely as might be to a bath of oil, and especially the effect of temperature on the co-efficient of friction. There is little doubt, I think—although I will not assert it positively, having simply the remembrance of general observations not specifically directed to that one point to guide me—that the difference between the temperature of the outside air and a journal bearing in good order in regular service is very much greater, perhaps several times greater, in winter than in summer. If so, and if all the energy lost, or substantially all, takes the form of sensible heat, the rapidity of radiation, and hence co-efficient, must be in close accordance with this difference of temperature, a possibility which, if established by experiment, might have the most important practical consequences. No experimental facts are now extant, so far as I know, in reference to co-efficients of journal friction at temperatures at or about 0° F., not even dynamometer tests of trains in service, that temperature not being a favorite one to choose for experimenting. Moreover, it is, I believe, a fact that the bearings of fast passenger trains are not found to be much, if any, hotter at the end of a run than freight trains at $\frac{1}{4}$ or $\frac{1}{5}$ the speed. This fact, in so far as it is precisely true, would require that the amount of heat radiated should vary precisely as the speed, a conclusion which, if established, has a possible bearing of importance on the probable loss by radiation from fast passenger engines. A fact tending to prove that the co-efficient of journal friction may be very much higher in winter than summer, is the enormous difference in coal consumption per train mile, and still more per car mile, in winter and in summer. A large body of statistics on this subject, which I hope shortly to put in shape for publication, seem to indicate with great clear-

ness that the fuel burned per car mile varies at the rate of about *two per cent.* per 1° F. of outside temperature, the difference per train mile being somewhat less. That is to say, months in which the average temperature differs say 20° or 30° will show a difference of about 40 or 60 per cent. in the coal burned per car mile, the coal per train mile being a percentage less, but both following with curious accuracy the monthly fluctuations in average temperature. No difference in average velocity of the wind exists tending to account for this, nor does the difference in radiation seem fully adequate for so very large a difference. If it be in part due to much greater journal friction at low temperatures, a proof of that fact might indicate the road to economies of great importance.

THEODORE COOPER, M. Am. Soc. C. E.—The paper is one of great interest, but, after carefully reading, it occurred to me that the writer has not taken into account all the elements of resistance in considering the power needed to start a car in motion. It would appear necessary to employ more power than is simply needed to overcome the journal friction. The measuring this friction, therefore, by the total power required to start a train would appear to give a too great frictional co-efficient. When at rest, the axle may be considered as receiving the weight of the car upon its highest point, as at *A* in accompanying sketch.



The outer circle in the sketch represents the inner surface of box, and the inner circle the axle (exaggerated). When the tension on the draw bar equals the amount to overcome the friction, the *position of*

equilibrium before motion of the axle and box will be on line $B\ O$, which stands to $A\ O$ at the angle of friction.

When motion takes place, the position of equilibrium will be on line $C\ O$, double the angle of friction from $A\ O$. The whole car would be lifted an amount due to this amount of motion.

So, in addition to the actual friction, the power to start a car must also be sufficient to lift the load the requisite height.

Some years ago, while examining a bridge, a coal train standing on the bridge, with the engine on solid ground, started. Every car, as it started, exerted a sudden blow upon the bridge and tried the structure severely. The only reasonable explanation seemed to be the idea above expressed, that each car had to be lifted a certain amount, and the reaction produced these blows.

Every one has noticed the backward motion of a car after it has come to a stop. It cannot be explained by any rebound from its position in a train, for it is equally true of a single car making a flying switch—as it loses its motion it comes to a stop, and then will roll backwards a little.

I have seen and had others observe the peculiar phenomenon of a wagon wheel flying backward as the wagon was moving forward, it usually occurring when the wheel passes from the rough ground on to a slippery layer of snow.

The rolling back of the car and the backward movement of the wagon wheel seems to point to the same fact, that the weight has been lifted an amount due to the friction, and falls back when motion ceases or the friction is reduced.

Charles E. Emery, M. Am. Soc. C. E.—What would be the effect, supposing the boxes did fit the journal?

Mr. Theodore Cooper.—The journals never do fit perfectly. Would you fit a journal so?

Mr. Charles E. Emery.—That is begging the question. Suppose the journal did fit ordinarily tight, would the car lift under such circumstances?

Mr. Theodore Cooper.—It would tend to lift.

Mr. Charles E. Emery.—It could not lift under such circumstances. The journal practically fits through an arc of 45 to 60 degrees; but if it fits for only 20 degrees (or for any angle greater than that of friction) no such action can take place, for the box, if it slides around the axle at

all, will simply turn on the centre of its arc of contact, which is the centre of the axle, and no lifting will result from that cause.

The lifting referred to, we have all seen, but it is to be referred to a different cause. The tipping of the trucks in stopping a train is due to the disturbance of the centre of gravity of the car, which being above the bearing pin, throws a little more weight on the forward springs of the trucks.

Mr. Theodore Cooper.—How do you mean that the car lifts?

Mr. Charles E. Emery.—I say that in stopping, the car body is thrown forward. The centre of gravity of the car as a whole, including the body, is above the trucks where the resistance is applied, so, in slackening speed just before the actual stop, there is a pitching forward of the body of the car that throws more weight on the front springs of the trucks than on the rear ones, which produces a rebound when the actual stop takes place.

Mr. Theodore Cooper.—How would you explain it on a car that had no trucks?

Mr. Charles E. Emery.—The car is a truck in itself.

Mr. Theodore Cooper.—Suppose the car was without springs?

Mr. Charles E. Emery.—The frame of the car itself is elastic enough to cause a rebound, with or without springs. The centre of gravity is above the centre of the wheels, and is thrown forward in stopping, and when the car comes to a state of rest the elasticity throws it back.

A. M. Wellington, M. Am. Soc. C. E.—I think the phenomenon of the sudden jerk at the instant of stopping is due to a different cause from that suggested by either Mr. Cooper or Mr. Emery, viz., the brief but remarkable increase in the holding power of brakes which takes place at the instant before coming to rest. The brake experiments of Capt. Douglas Galton and Mr. Geo. Westinghouse showed very conclusively that the co-efficient of friction was, for that instant, several times (five to eight times, if my recollection serves) greater than the normal, following in that respect very exactly the general law of friction which the experiments detailed in this paper indicate for journal friction. No doubt it is in each case due, not to the mere fact of stopping of starting, but to the slow velocity. If the same velocity continued, the same high co-efficient would probably continue indefinitely.

Now, this explains perfectly the phenomena alluded to. The car-body is moving through space without friction, the retarding force

coming from the truck. While this force is uniform the car-body at least moves smoothly, but at the very instant of stopping the sudden augmentation of break energy is so great that it is hardly an exaggeration to say that the effect is much the same as if the car ran at that instant against a slightly elastic fixed buffer. It is for this reason that passengers standing in the aisles of cars coming to a stop stand very comfortably until the last instant, when the stop is consummated, when they almost invariably lose their balance more or less. Their bodies are adjusted to an angle corresponding to the resultant of gravity and the *normal* retarding force. The sudden augmentation of the latter disturbs their balance.

Mr. Theodore Cooper.—This does not explain the running backward.

Mr. A. M. Wellington.—It appears to me that it does perfectly. It is a slight elastic rebound, the same in its nature as if from a fixed buffer, when it occurs at all, which is not always.

Mr. William H. Paine, Vice-Prest. Am. Soc. C. E.—It would seem to me that Mr. Wellington's testing machine illustrates an idea I thought of at the time it was brought out. The very friction tends to throw a pressure on the springs in front that might relieve the springs behind. When you stop, you right this, and the car coming to an equilibrium produces what might be called a backward reaction.

Mr. F. Collingwood, M. Am. Soc. C. E.—It is undoubtedly true, as stated by Mr. Emery, that a decided tendency to rotation about the forward truck of a car is always developed in coming to a stop, which would *partly* explain the motion noticed by Mr. Cooper, and is *sufficient* to explain the shock to a bridge from the starting of a long coal train; as the downward component of the force in a short car would be much greater than in a long one. Mr. Wellington's statement also as to the rapid increase in the co-efficient of friction as the motion diminishes seems to be well established. If, then, the work of friction is increased, how is it taken up? It can only be by the elasticity of the rods, which, at the last portion of the forward movement, undergo an increase in tension, and so soon as all movement ceases give a positive rebound. These two causes, the rotation of the car-body forward, and the rebound of the rods from a state of high tension, seem sufficient to explain the phenomena mentioned.

Mr. A. M. Wellington.—There is a slight vibration of the car produced by release of the brakes, for obvious reasons; but I think that phenomenon occurs after the one in question and has no real connection with it. Mr. Cooper's suggestion of a lifting of the car is undoubtedly a theoretical possibility, but bearings ordinarily fit so nicely for the angle which they cover, and the angle of friction even in starting is so very small at best (about 0° 35' for 20 pounds per ton resistance), that I do not think it is the true explanation of the jar he speaks of.

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ON THE REAL VALUE OF LUBRICANTS AND ON THE CORRECT METHOD OF COMPARING PRICES.

By ROBERT H. THURSTON, M. Am. Soc. C. E.

READ JANUARY 7TH, 1885.

The real value of any lubricant is a quantity which seldom has any direct relation to its market price, and depends not only upon the intrinsic qualities of the unguent itself, but upon the economical conditions under which it is to be used. It is dependent to a greater extent upon the magnitude and cost of power than upon the expense of its purchase or preparation for use by the consumer. The correct method of comparing prices, from the user's standpoint, is not one involving merely a determination of the properties of the material as a reducer of friction, and the true value of the oil is not simply proportional to its endurance and its power of reducing lost work; it includes a study of the method by which it reduces the total expenses of lessening friction, and the extent to which total expense for power is reduced by such reduction of work wasted by friction. The usual systems of comparison are entirely wrong, and are only justifiable by the fact that hitherto it has been impracticable to obtain the data required for the establishment

of a correct method. This difficulty no longer exists, and every intelligent purchaser of lubricants is coming to see that he may often effect enormous economies by the careful study of the variation of the total cost of lubricant and of waste power.

The total cost of the lost work in machinery includes two distinct items—the cost of lubricant, and the cost of doing the work of overcoming friction of the lubricated surfaces. Of these, the latter is usually enormously the greater, and it is at once seen that a saving in cost of lubricant is of slight importance in comparison with a saving of equal proportion in the reduction of the cost of the power demanded to overcome friction, and which is thus wasted. A dollar expended in the substitution of good oil for one of lower grade may save a hundred by reduction of the waste of fuel and other expenses of power-production. Such expenses include fuel, salaries, interest on capital invested in motive power, taxes and insurance on the driving machinery, boilers and building, and other and minor costs, which every proprietor can readily estimate with fairly accurate figures, if not with perfect satisfaction to himself. The total cost of steam-power thus foots up to about $100 per horse-power per annum in New York City, and to a minimum of, perhaps, $50 under more favorable conditions. Water-power often costs considerably less, although the cost of dams, reservoirs and machinery is large.

If, in any case, we call the total expense per hour K, the cost of the lubricant on the journal k, the quantity used q, the total cost of power per horse-power per hour k', and the amount of power used in overcoming friction of lubricated surfaces U, the total expense chargeable to "lost work" will be

$$K = k\,q + k'\,U \qquad (1.)$$

The work done in overcoming friction U, is proportional to the mean pressure on the lubricated surfaces P, to the speed of relative motion of rubbing surfaces V, to the time taken for comparison t, and to the magnitude of the co-efficient of friction f. Thus we may write

$$K = k\,q + b\,f \qquad (2.)$$

in which b is a constant, the value of which, $k'\,P\,V\,t$, is easily ascertained in any given case, and the calculation of the cost of friction is then readily made.

Where two oils are to be compared, to determine the economy to be secured by the substitution of the one for the other, the values of q and of f, and the cost per gallon, k, of each will be known, and the two values of K thus obtained will exhibit the relative economy of their use. If K is the same for the two, it is a matter of indifference which is used; if K is greater in one case than in the other, that oil is the more economical which gives the lower value, even though it may cost more per gallon, and may require to be more freely used than the other. Thus, suppose, for the two cases we have

$$K_1 = k_1\, q_1 + b f_1\,; \quad K_2 = k_2\, q_2 + b f_2$$

If these two values of K were equal, $K_1 = K_2$, and the gain by purchasing of the second oil is just compensated by the loss due to increased demand for power to overcome the increased friction, and

$$k_1\, q_1 - k_2\, q_2 = b\,(f_1 - f_2) \qquad (3.)$$

$$k_2 = \frac{k_1\, q_1 + b\,(f_1 - f_2)}{q_2}. \qquad (4.)$$

Any price paid for the second oil, *delivered on the journal,* less than k_2 gives a profit; any greater price produces loss. This last equation is thus a criterion by which to determine what price, k_2, may be paid for any oil proposed to be substituted for the first oil, costing $k_1\, q_1$ per hour.

Where the same quantity is used of each, as may be the case frequently, $q_2 = q_1$, and

$$k_2 = \frac{b}{q_1}\,(f_1 - f_2) + k_1 \qquad (5)$$

The question sometimes arises whether it is better to use a larger quantity of a certain oil already in use; in this case $k_2 = k_1$, and the quantity allowable without loss is

$$q_2 = \frac{b}{k_1}(f_1 - f_2) + q_1 \qquad (6.)$$

Where the relative endurance, and the relative values of the co-efficient of friction, are determined by experiments made under the conditions of proposed use, if e and h represent the two ratios, since the quantity used will be inversely as the endurance, and the power wasted will be directly as the co-efficients of friction,

$$k_2 = e\, k_1 + b\, e f_1 \frac{1 - h}{q_1}\,; \qquad (7.)$$

and this expression becomes the criterion of values.

Instead of taking the time as one hour and the unit of power as the horse power, it may be convenient to adopt other units. Thus, on railroads the costs are measured by the cost of oil and of power per train-mile, and

$$K = k\,q + d\,f \qquad (8.)$$

in which q is the quantity of oil used per mile, and $d\,f$ is the cost of power for the same distance. Also, as a criterion,

$$k_1\,q_1 - k_2\,q_2 = d\,(f_2 - f_1)\,;\ k_2 = \frac{k_1\,q_1 + d\,(f_1 - f_2)}{q_2} \qquad (9.)$$

In illustration of the application of these principles, take the following cases:

(1.) The proprietor of a large machine shop informs me that he finds the total expense of power to be nearly \$100 per horse-power per annum, of which power one-half is estimated to be expended in doing work wasted in friction; that he uses 0.02 gallon per hour of good lubricants, costing an average of \$0.50 per gallon. The mean co-efficient of friction is judged to be about 0.05. The value of b (eq. 2) is found to be 0.6 horse-power, or 30 for 50 horse-power; then

$$K_1 = 0.01 + 1.50 = \$1.51.$$

Suppose it be proposed to substitute for the oil in use one which costs but \$0.25 per gallon, and of which 0.03 is required per hour, and that the co-efficient of friction with the cheaper oil is $f_2 = 0.06$; then

$$K_2 = 0.0075 + 1.80 = \$1.80\tfrac{3}{4},$$

and a gain of one-quarter of a cent per hour, or \$7.50 per year, is effected at the expense of a loss in cost of power of 30 cents an hour, or \$900 per year, and a net loss of \$892.50.

(2.) A cotton mill, using 200 horse-power, in work of overcoming friction of lubricated surfaces, uses 0.7 gallon of oil per hour, at \$0.70 per gallon; it is proposed to substitute an oil costing \$0.40, and of which one gallon per hour will be required to do the work, while the co-efficient of friction will rise from an average of 0.10 to 0.12. Taking b at 60, as before :

$$K_1 = 0.49 + 12.00 = \$12.49;$$
$$K_2 = 0.40 + 14.40 = \$14.80.$$

A gain in expense for oil amounting to 9 cents per hour, or \$270 per year, produces a loss in cost of power of \$2.40 per hour, or \$7 200 per

year, assuming 3 000 working hours per annum. The net loss is \$6 930, *i. e.*, nearly 30 times the profit on the oil account. This is not an unusual or an extraordinary case, as matters are now going on in the business.

(3.) A railroad train requires 1 cent's worth of oil per mile, and costs 10 cents per mile for power expended in friction, using a good oil, costing 50 cents per gallon, at the rate of 0.02 gallon per mile, with a mean co-efficient of friction of 1 per cent. It is proposed to change, using an oil costing but 25 cents, at the rate of 0.03 gallon per mile, and obtaining a co-efficient of $f = 0.015$; then

$$d f_1 = 0.10\,;\ d = 10 \text{ (eq. 8)};$$
$$K_1 = 0.01 + 0.10 = \$0.11\,;$$
$$K_2 = 0.0075 + 0.15 = \$0.15.$$

In this case, a gain of one-quarter of a cent per train-mile in cost of oil brings about a loss of 4 cents—sixteen times as much—in increased train resistance.

Using the equations given as criteria of values (eq. 4, 6, 9), we find the estimated value of k_2 to be, in the three cases given, respectively: — \$19, — \$2, and + $16\frac{2}{3}$ cents, nearly, for the cases as taken. That is to say: the proprietor of the machine shop will lose \$19, nearly, on every gallon of the proposed oil that he may use; the owners of the cotton-mill will lose about \$2 on every gallon of the inferior oil that they may purchase; while the railroad will lose money, unless it can get the second oil for $16\frac{2}{3}$ cents.

But suppose, in further illustration, that it is found possible, by increasing the supply of oil in the case of the machine shop, to reduce the mean co-efficient of friction to 0.02, by using four times as much of the cheaper oil as was, at first, thought advisable. Applying our criterion to this case, we get (eq. 4):

$$k_3 = 0.03 + 0.60 = \$0.63\,;$$

and a gain is effected of nearly two-thirds the original cost of lubrication. An expenditure of \$60 gives a profit of about fifty times that amount.

It must not be assumed that these figures are more than rough approximations to fact; for it is difficult to obtain exact values of the quantities involved, and especially of the true mean value of f; but they are sufficiently correct to answer as illustrations of the principles involved,

and are near enough to the truth to give a fair idea of the magnitude of the losses which are each day met in consequence of the practice of the system of false economy now generally practiced in the purchase of lubricants.

The values assumed for the co-efficients of friction are probably fairly representative of those found in common practice. The experiments made by the writer show that, under ordinary conditions of every-day practice, the value for mechanism working under as light pressures as are met with in spinning frames, for example, different oils will give values varying from 0.10 to 0.25; under the usual pressures of heavy mill-shafting, the figures range from 0.5 to 0.10; with pressures of greater intensity, such as are met in the steam engine and under railroad axle bearings, it often varies, using different lubricants, from about 0.01 up to 0.025, the first value being given by the best oils and the second by heavy greases. Under the exceptionally high pressures and at the speed of rubbing reached on the crank-pins of some steam-engines (500 to 1 000 pounds per square inch, 35 to 70 kgs. per sq. cm.), f may fall to one-half the last given values. In endurance, the same variations are met with. The endurance decreases as pressures increase, and is twice as great with the best oils as with others of good reputation. The market prices of oils have no relation to these relations of quality. The best oils for any given purpose may be either more costly or cheaper than others less well fitted for the work. In some cases prices are made in the most arbitrary manner.* Sperm, lard, olive, and some few standard grades of mineral oils probably have fair and well-settled values; as a rule, however, the price of a mineral or of a mixed oil is no guide to selection.

Should time permit and statistics prove to be attainable, the writer will endeavor to develop this subject more completely.

* The writer has been informed of one case in which the dealer purchased an oil for 12¼ cents per gallon, gave it a trade name and sold it, unchanged, at $1.25. It was worth that amount, however, if compared with other oils in the market that may have cost the "maker" much more.